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UNIVERSITY CONTROL[1]

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AMPLE justification for further consideration of problems connected with university control exists in the vastness of the interests concerned. The commissioner of education, in his last report, states that in the United States there are 607 colleges and universities, with almost 22,000 instructors and approximately 118,000 students; with property, real and personal, valued at $465,000,000 and an income in 1904 of $40,000,000. All recognize that the management of our colleges and universities as purely business enterprises is almost beyond reproach; but there is no such consensus of opinion respecting the administration of the trust itself, many believing that this is wasteful and inefficient, while some seem ready to assert that good faith has not been kept toward donors, many of whom had no definite conception of the work for which their money was given, but had confidence in the wisdom, integrity and qualifications of those to whom they entrusted the gifts.

The American university is a corporation managed by a board of trustees, often self-perpetuating, which, according to the state law, controls all details of management. The vast material interests have made necessary a separation of business affairs from those of educational work and control over the latter has been concentrated in the hands of a president, who gradually became director of the whole organization, determining not only its educational policy, but also, in

[1] Articles bearing upon this subject have appeared recently by Presidents Draper and Pritchett in the *Atlantic Monthly*, by Mr. J. B. Monroe and Professor Jastrow in *Science*, by Professor Cattell in *Science* and the *Independent*, and by Professor Stevenson in the Popular Science Monthly, all of which have been utilized in preparation of this article.

not a few instances, dominating even the financial affairs. The arguments justifying this evolution are plausible. Experience shows that in every organization, left to itself, some one man, through native force, gains control. University trustees should not permit this matter to adjust itself as the one, thus gaining control, might be swayed by wrong motives or might be ill-balanced—in either case injury would come. Far better for the trustees to select some man of all-around fitness and to recognize him as the responsible head. Acting on this principle, trustees appoint as responsible president one who from their standpoint possesses the necessary qualifications and make him practically attorney in fact for the board, giving him free hand in all departments of the work.

Advantages of the American Plan

That this procedure is good appears at once by comparison of conditions prior to the civil war with those at this time. In the earlier days, when the autocratic system existed only in germ, the resources of colleges and universities were small and increased slowly. Buildings, for the most part, were uninviting and students were few. The faculties, in most cases, were small but made up of strong men, faithful teachers, fruitful investigators. Salaries were modest, but the social conditions were equally modest, and the professor's position made up in honor what it lacked in pecuniary reward. The equipment, even in what are now great universities, was insignificant; a professor desiring to make investigations in physics or chemistry, either purchased or manufactured the necessary apparatus, while another, pursuing special studies in any branch of literature, spent his savings in gathering material. Too often the college provided rooms for teaching, the instructor provided the rest. Yet it must be conceded that the colleges did admirable work. They imparted not a great deal of knowledge, for the courses were very narrow, but there was a system in the training which sharpened many a dull intellect and made the already sharp intellect keener. The purpose confessedly was not to impart knowledge, but to train the intellect, to fit the man for professional study.

All was changed after the civil war. The material needs of the country demanded opportunity for a new type of training, adapted to the needs of men with wholly different aims. This required chiefly the imparting of knowledge with intellectual training as subordinate; not cultural studies, but studies in applied knowledge. Technical or semi-technical schools were established, and wealthy business men, on their own initiative, gave vast sums for such schools. To retain their place, the universities quickly developed along the same lines,

placing technical schools alongside of those for law and medicine. This work of expansion was placed in charge of the president, who, under pressure of the new responsibilities, soon ceased to be teacher and became merely administrative officer. The splendid results of this policy are visible everywhere in all departments of our universities. Instead of simple factory-like buildings, imposing fireproof buildings surround the campus, which, in its turn, is no longer a grass plot, mowed two or three times a year, but a beautiful park; the library building is a credit to the architect and the shelves are well filled; the gymnasium is usually a noble building, a proof of anxiety for the physical well-being of students; the laboratories are equipped elegantly and abundantly; the museums are impressive; the mechanical workshops are marvels of completeness; students, in the old as well as in the new courses, formerly counted by scores are counted now by hundreds, and the number of instructors has increased proportionately; in material resources, the unit is no longer tens of thousands, but hundreds of thousands of dollars; the gifts to educational institutions during the last forty years make a sum so vast as to be almost incredible. The history of college growth in material resources during the last four decades is like a leaf from the Arabian Nights and Aladdin's lamp seems no longer a fairy tale. This history tells, too, of devotion and suffering on the part of some college presidents as noble as that of the early martyrs, and deserving a measure of honor which will never be given, as theirs was the day of 'small things.'

Disadvantages of the American Plan

But all this is only one side of the picture, that which presents itself to a merely casual observer; it is the purely material side. There is another side, not so patent in some of its aspects, yet so apparent in others that even the newspaper humorist, that most casual of observers, has not failed to detect and to utilize it. The thoughtful observer, familiar in some degree with matters of education, is led soon to doubt if, in this great development, the interests of education have been regarded as paramount. He asks respecting endowments, and learns, not altogether to his amazement, that in the rush they have been overlooked; and he may learn too that by the acquisition of buildings, the available resources of the college have been lessened, as both giver and receiver failed to provide for maintenance; he may discover also that in carrying out plans of one sort or another, obligations were assumed before means were secured to meet them. It is wise to examine the other side somewhat minutely.

Effect on the Trustees.—On one hand, the growth of financial interests has made compulsory the appointment of successful business

men to the boards, while the urgency for additional funds has led to selection of men very prominent in all callings—extremely busy men. On the other hand, the extraordinary growth, internally, of the colleges and universities has made no longer possible for the trustee that familiar acquaintance with professors and with the departmental needs which he ought to have. Ordinarily, one finds in a board two or three members who become so attached to some school or department as to give genuine attention to its affairs and who do gain much information respecting it; but most of the others have no leisure, or think they have none. To make this isolation complete, there is no official communication with the faculties except through the president, for cases are very rare in which the faculties have representatives in or before the board of trustees. Unfamiliar with educational affairs, unacquainted with the needs of the college under their care, too often without personal knowledge of the professors or their qualifications, these trustees select a president. Recognizing their inability to perform the duties devolving upon them under the law, they practically transfer their responsibilities to their appointee, and thereafter their principal function seems to be simple legalization of his acts. Although the average trustee of to-day is a far abler man than his predecessor of a generation ago, circumstances have made him far less efficient as trustee; in too many instances he is director in name only and many men seem to assume the office with as little sense of responsibility as though they were to be directors in a corporation of which one man holds a controlling interest. The creature has become greater than his creator and the board of trustees has lost even its old-time efficiency as 'a pipe-line for shekels.'

Effect on the President.—Formerly, the president was to all intents simply a professor with some additional responsibilities for which he received additional remuneration. But the president of this day is very different. His duties have been summed up recently by Dr. A. S. Draper, and the catalogue as given is sufficiently interesting to deserve at least partial reproduction. The president must

> see that the property is cared for; that the teachers are efficient; that proper men are found to fill the chairs; that the institution's work is organized properly; that the resources are assigned rightly to the several departments
> Decide the lines along which the institution should develop; uphold proper ideals and make them attractive to real men—old and young; be forehanded and peer into the future; initiate policies; puncture fallacious logic and much of it; augment the resources of the institution; make the whole efficient for increasing service; manage and guide students, who must be dealt with individually; construct as well as administer; declare the best university opinion concerning popular movements and serious interests of the state; connect the university with the life of the multitude; exert university influence for quickening and guiding public opinion; be able to work harmoniously with others;
> but he must work out his official course for himself.

As Dr. Draper is not speaking of the small college with one hundred students, no one will be disposed to dispute his assertion that the position calls for a rare man. It may be added that a board of trustees competent to make intelligent choice of this rare man would be composed of still rarer men. If they should be fortunate enough to find him they could not keep him, for such ability is in demand, and some life insurance company would soon offer him several times the salary for a small fraction of the work. Undoubtedly Dr. Draper has summarized the requirements as they are idealized in some minds and no doubt there are men who feel assured that he has described the work they actually do, the whole work of the trustees as well as the whole internal work of the institution aside from teaching. Beyond all question, there are those who attempt this. But appointments are not made on the basis of this rare, broad qualification. The only question is as to the candidate's ability to meet the requirement which the board thinks most urgent—usually one which in the list seems to be of rather secondary importance. And one may not censure the board for this. As the number of colleges is far beyond the country's needs, financial stringency is ordinarily the only requirement with which the trustee is familiar. The selection, as a rule, is not made because the candidate is qualified to control an educational institution, but rather without any reference to that matter. As a rule the appointee is not a teacher. He is apt to entertain great respect for education and none too much for educating or educators.

The newly-appointed president may or may not have an ideal—that is unimportant. He finds quickly, except in some of the older universities, that the board of trustees has an ideal; that board expects a commercial success, more money, more students. The president's path is marked out for him; he is not to be successor to Hopkins, Witherspoon or Day; he is to be a wandering mendicant, exposed to rebuffs and disappointments of the most galling type; he is to feel that prospective heirs look on him as attempting to rob the widow and orphan. However sharply one may assert that the president's office, as it now exists, is an injury to higher education, he must recognize the heroic sense of duty which prevents so many presidents from abandoning their posts.

The most serious matter in this connection is the complete alienation of the president from the work of teaching. In the smaller institutions, where he is still professor of some branch of philosophy, his work as teacher is wholly subordinate to that as traveling collector of funds. In the larger universities, teaching is impossible, and the president is simply managing officer of a great corporation, with buttons on his desk which keep him in touch with managers of departments. His work is purely administrative, and in the very nature of the case he comes to regard all within the corporation's range as his subordinates. If he

have had thorough training as a college professor, it is possible for him to retain some touch with the educational side; but if, as is usually the case, he have had no such training, his interest in educational matters is apt to become purely academic. And such a condition is fully in accord with the popular notion respecting the president's duties; the position needs a man of great executive capacity, great energy, magnetic personality, capable of keeping himself in public view, so as to advertise the university, to attract students and to increase the resources. Recently a new president was chosen for a promising young college. Interviews with trustees and others appeared promptly in the newspapers stating that with this man's magnetism, the institution will have a million dollars and a thousand students within ten years—not a word about education or elevation of grade. A notable illustration is the frequent reference to President Roosevelt as the proper successor to President Eliot of Harvard—though every thoughtful man at all familiar with university needs and objects must recognize that President Roosevelt, with all his remarkable ability, has not the qualifications required for control of a university, large or small.

Yet this officer, becoming every year less and less fitted to preside over educational affairs, becomes each year more firmly fixed as autocrat, for, if at all successful in raising money, he soon develops into the administration. The trustees may be restless, when ignored, but that is unimportant, for they know very little about the institution, while ordinarily the trespass upon their prerogative is so gradual that no new advance is sufficient to justify decisive action. The president, originally a lawyer, clergyman or business man, has sole power over appointments of professors, over the fixing of their salaries and over the curriculum itself, for he may establish a new chair at any time. It is not too much to say that the office of college president, as it exists in most of our colleges and universities, is the great menace to higher education in America.

Effect on the professors.—The all-essential portion of the university is the teaching staff; it does the work for which the college or university was founded; all other portions of the organization, trustees, president and 'what not' were intended for the encouragement and strengthening of this staff. Under the American system, the relations have been reversed.

There seems to be a deliberate attempt to convince the community that college professors are singularly child-like in simplicity and in lack of business capacity. One president has dilated on the unworldliness of college professors, and has left the impression that he thinks low salaries not altogether bad as they tend to encourage high thinking and indifference to worldly affairs. Another describes the ideal trustee in glowing terms, he stands transfixed while contemplating the majesty

of the president, but in the professors he finds only material for ridicule. Teachers as a rule are impractical; faculty meetings seem to be but burlesques, and he clinches his description of unfitness by the broad assertion that faculties can not be entrusted with the selection of professors. In reading such statements one can not repress amazement that men so efficient as many college professors are in executive work, in political affairs, in corporations of many types, should lose all as soon as they come into contact with their life's work. At the same time he finds comfort in remembering that some important colleges in this country exist to-day only because professors assumed the business burden when trustees had thrown it down in despair; and he can not forget that the most successful presidents, judged even from the ordinary standpoint of success, were chosen from the faculties of the colleges over which they preside. One is at least safe in asserting that the training of college professors in business matters is quite equal to that of men in the clerical profession, from which so many college presidents have been selected.

Both trustees and presidents act on the principle that professors need guardians. The college faculties, especially, are practically ignored; little by little their authority has been curtailed until now it extends little beyond the class-room. In some of the larger institutions, faculties no longer choose their officers. Faculty meetings in some departments are unimportant affairs, and professors attend them as they perform other unimportant things, because they are on the list of duties. Certainly the meetings are characterized by pointless discussions, but this is due to the presiding officer, the president himself or his representative, who lets go his hold on the tiller and leaves the craft to wander at will. But there is no reason why the discussions should be other than aimless; decisions carry no weight except in matters wholly insignificant. The board of trustees in its innocence is available to correct any erroneous decision. Professor Jastrow refers to a case in which the faculty was informed that its action was a matter of indifference, that the trustees would decide the matter as the president wished. The writer has learned of another case in which the faculty received no such preliminary information, but was permitted to waste its energies in long and careful consideration of a proposition involving an important principle. Not many days after the decision was reached, the faculty was called together to receive information that their action had been overslaughed by the trustees. How much interest or importance should attach to faculty meetings is not difficult to comprehend.

Some newspapers have much to say respecting subordination of professors to millionaires who have given large sums to colleges. The writer has found none of this among professors and he has yet to find

the giver who has shown desire to meddle with 'professorial freedom.' But there is a subordination which the writer knows leads young men to despise the professor's calling, leads them, in Mr. Monroe's words, to look with scorn upon a calling in which the individual is annihilated. The president, too often a graduate from one of the narrowest courses in college, too often belonging for much of his later life to a dogmatic profession, has power to make and does make appointments on his own motion to chairs in literature, philosophy as well as in pure and applied science; he controls promotions and salaries; the professors are subordinate to him individually as the faculties are collectively. Young men, knowing the conditions, refuse to enter the calling; others, ignorant of them, spend years in preparation and enter the calling only to find, when too late to escape, that their ideal was as a will o' the wisp.

The assertion is made that, were it not for this control, professors would become perfunctory in their work. Some kind of control is needed even for the best of men that matters be not at loose ends, but it should be intelligent to be efficient. A successful business man, an eminent lawyer, a great clergyman, would not prove efficient as superintendent of a shop for grinding microscope lenses or for manufacturing chronometers. And if occasionally he expressed opinions belittling the skill required for the work or showed preference for quantity rather than for quality of work, his control would be of doubtful value. This is a condition in too many colleges, with the result that the president is on one side, the faculty on the other, with nothing but distrust in common.

It is strange that so few college professors become perfunctory in their work. They receive little personal recognition. If they exert themselves to build up the library, museum or apparatus, if they induce an acquaintance to give an endowment, all these are so many packages thrown into the president's basket of achievements. Their services are not acknowledged even in a material way. Their salaries are petty; the salary of a rowing coach in a great university is larger than that of an assistant professor who has done efficient work for many years; in case of urgent deficit, the first relief suggested is in reduction of the professors' salaries. In other professions, experience and efficiency lead to promotion; in this other matters prevail, and too often a young man, untried, is appointed at higher salary than that received by older men of well-ascertained efficiency. It is surprising that so few men come to share the apparent opinion of president, trustees and many students that their work is of only incidental importance. Yet there is no reason why college professors should be more transcendental than other men.

These statements may seem strange to many persons of wealth. The needs of the 'poor self-denying professor' are exploited so fre-

quently by college presidents in their appeals to generously-inclined people that the 'poor professor' is almost a byword. In truth the professor is often poor enough, but he is not guilty of exploiting his poverty or of seeking praise for self-abnegation. In any event, he has profited little from large gifts, which too often take the shape of buildings, thereby increasing the running expenses and endangering the already too small salaries. There is, indeed, sign of awakening conscience, for one day last summer it was announced that a college had received a considerable sum of money and that the salaries would be increased at once. Harvard has received a great sum, whose income is to be devoted solely to endowment; while the presidents of two other great universities have announced that increase of salaries is the most urgent need. It must be remembered, however, that these universities are in large cities, where the salaries, though counting large in dollars, have comparatively small purchasing power. Five thousand dollars in New York city is actually less than two thousand in many a college town, while two thousand dollars in that city means living in conditions incredibly narrow to dwellers in villages. This matter of salary is, however, relatively unimportant. The all important matter for consideration is the insignificant position of the professor in the organism of which he is the all-important element. These words are written with due deliberation. During his almost forty years of service, the writer has seen the gradual evolution of the president in American colleges and the resulting decadence of the professor.

Effect on Higher Education.—The American university is a great business corporation, conducted on business principles. The sense of ownership is as marked in president and trustees as though the corporation had been formed to make drugs or to build ships and they held all the stock. Within a few months, we have seen the spectacle of two educational corporations endeavoring to unite their properties under one control, though the faculties were opposed to the union. Intervention by the courts was necessary to prevent consummation of the deal. A few years earlier, negotiations of somewhat similar character were conducted between two other institutions, without any reference whatever to the faculties' opinion—properly enough, too, if, as stated by one of the trustees, the professors are merely employees of the corporation. The justification for such procedure is that men outside of boards of instruction see things from a higher plane than do those inside. One must refrain from commenting on this plea.

The anxiety to have the corporation do a big business makes number of matriculants quite as important, to say the least, as the character of instructors or instruction. Summer schools, at first mere incidents, are now recognized parts of several universities, and even modest colleges are not without them. They are important, affording opportunity for

instruction in all subjects from Greek up or down to kitchen-gardening and dancing, affording great opportunity for cultivating the social side and adding notably to the list of matriculants. Appendages affording side passages to degrees are as welcome as summer schools, as they benefit a worthy class and add to the matriculant list. The correspondence school has not gained full recognition, but the importance of the others justifies the hope of its founders. This type of expansion has been at the expense of efficiency. New schools, new courses, are added, the catalogue becomes more bulky each year, but the number of instructors is increased in small proportion. The instructors become mere lesson-hearers. In one institution, professors offer twenty to thirty-one hours a week of actual class-room work in various schools. How much energy remains for genuine study is not difficult to determine. One need not wonder that college professors no longer lead in investigation and discovery. This anxiety for bigness has led to the prominence of semi-professional athletics, to the lowering of standards that college champions may 'get through,' to the lowering of ideals and even of morals. A student expressed well the general sentiment of his class when Columbia took its stand against certain forms of athletics—"What is Columbia coming to anyway? It's going to be nothing but an educational factory."

Conceding all that is claimed for the present system, the question still remains, Has the gain equalled the cost?

No candid man, who has examined the subject carefully, who has studied many colleges, will answer the question affirmatively. It matters not how firmly he is attached to the present system, he must acknowledge that the results, from an educator's standpoint, are not commensurate with the expenditures—more, that in some directions it has led to positive waste. If one look over a pile of college catalogues from different parts of the country, he will find whole broods of academies masquerading as colleges, even as universities, with one twentieth to one fourth of their pupils taking college studies, with a long list of teachers, with a president traveling over the country, prating on the advantage of the 'small college,' pleading the cause of the 'poor professor' and working on denominational prejudice to make good the annual deficit of which his salary and traveling expenses form a large part. There is something wrong in a system which creates a public sentiment such as permits a half-million dollar gymnasium or an immense stadium for semi-professional intercollegiate contests to be heralded as a gift to education; that receives gifts for scholarships and fellowships with as much acclaim as gifts for endowments; that points to piles of masonry and to mere lists of matriculants as proofs of success, that places the college on the basis of the shop and proves economy of management by showing as many clerks as possible on a

minimum of expenditure. As far as true educational work is concerned, it is not too much too say that a very large part of the gifts might as well as not have been withheld.

Suggestions Looking to Reform

As has been said many times respecting other matters, things have reached such a pass that a change for the better must come soon. But the change will not come of itself, it must be brought about. Some have suggested that there be frequent consultations of college presidents; others, that the presidents and representatives of boards should meet for conference. But there is no promise of relief in such suggestions; whatever of promise there is looks rather toward making matters worse. There is no possibility of change for the better until there is full recognition in practise of the academically undisputed fact that the university in its essence is educational, all else about it being purely incidental. With this will come recognition of another fact, that no one should be entrusted with the executive duties of a university or college unless he have had as thorough training for the post as that required of bank presidents. It will be recognized also that choice of this executive officer should be made by those whose special training has fitted them to judge respecting the qualifications of a candidate. A board of clergymen and college professors, no matter how eminent they might be, would not be thought competent to select a president for one of our great banks. Even before recognition of these facts, men should see that no return to a proper ideal is possible so long as the whole policy of a university is dictated by one man. Recent explosions in the business world have proved this true for commercial corporations; it is equally true for educational corporations, more important than the others, in that their influence is not local and temporary.

The teaching staff must be recognized as the all-important part of the institution, for whose support and encouragement all other parts exist. The presidential wedge, now constantly widening the gap between the business and the educational interests, must be removed and the gap closed. The business man and the teacher must be brought into contact, the inevitable result being, as Mr. Monroe has said, great profit to both.

The organization of many universities is so complex that genuine re-adjustment can not be effected rapidly and a *modus vivendi* is necessary during the interval. This is possible with merely statutory changes. Faculties should elect their own officers. No change in the curriculum or assignments, no subdivision of chairs or creation of new ones should be made except upon recommendation of a committee for each faculty, consisting of trustees appointed by the board and of professors elected by the faculty concerned. Equally, no appointment to

the teaching staff should be made except upon recommendation by the same committee. In respect to matters affecting the relations of two or more faculties, the trustees should act only on the recommendation of a senate consisting of the deans and a representative elected by each faculty. All faculty elections should be by ballot without previous open nomination.

But this is merely palliative; organic change is necessary to secure permanency.

Edinburgh university, burdened during a long period by unintelligent control, gained freedom after a severe struggle in which the faculties were led by Sir William Hamilton. The conflicting interests were so many that reorganization was a much more complicated problem than that for our universities. The corporate body of the university is the university court, answering to the American board of trustees, as it has final control over all matters, except appointment to certain chairs, which are under patronage. A general council, consisting of certain officers, the professors and such alumni as have paid, once for all, a stipulated fee, elects a chancellor and four members of the university court. The chancellor is not a member of the court but appoints a representative. All acts of the court involving organic change must receive his sanction, but aside from this he seems to have no serious responsibility. The Senatus Academicus, consisting of the professors, elects four of its own number to the court; the individual faculties elect their own officers and control their internal affairs, subject to appeal to the court. The matriculated students elect a rector, who is presiding officer of the court, to which he appoints a member. The Town Council of Edinburgh elects one member of the court and a principal, whose chief duty is to preside at meetings of the senatus. There are some other regulations due to peculiar conditions surrounding the university, but they are immaterial here. Under this new system, the resources of the university have increased enormously, the courses of instruction have ceased to be medieval, while the number of students, in spite of constantly increasing severity of requirements, has multiplied several fold. This method has the merit of utilizing to the best degree all groups connected with the university, while the faculties are brought into close touch with each other and with all matters affecting university interests.

Many suggestions have been made with especial view to American conditions. Professor Cattell has summarized these, making important additions of his own. The essential features of his presentation are:

The corporation should consist of professors, alumni and members of the community—in the case of a state university, the people should elect part of the corporation; trustees, chosen by this corporation to care for the secular affairs, should elect a chancellor and a treasurer. The

professors should elect a president, a man with expert knowledge of education and university administration, whose salary, dignity and powers should be similar to those of individual professors. The university should be divided into schools as units, each of which should elect its dean and executive committee and should nominate men to fill vacancies. These nominations should be subject to approval by a board of advisers, consisting of professors from the school concerned, from related schools and from outside of the university; but final election should be by the university senate, and subject to veto by the trustees. Each school should have educational as well as financial autonomy and its property should be a trust fund for its benefit. Representatives from all schools should be elected to a senate, which should legislate for the university as a whole and should be co-ordinate with the trustees.

Reorganization along the lines of either method would bring about the desired result. The faculty's influence would pervade the whole institution and the work throughout would be directed to educational ends.

Undoubtedly were such changes effected others would come about. In all probability, catalogues, for a time, would show a decreased number of matriculants and possibly the total of benefactions reported to the commissioner of education would be diminished seriously. The writer has long believed with Professor Jastrow that decrease in benefactions and in the number of matriculants would not be serious evils. While the number of matriculants might be less, the proportion of students would be more and while the total of benefactions might be less, the amount devoted to genuine educational work would be more. The number of colleges might be reduced—certainly no calamity. Many so-called colleges, described by the editor of a prominent religious paper as academies with a college annex, might disappear. As academies with a few teachers, they could pay their way—with their petty college annex, they have a deficit. These gone, there would be relief to the generously-inclined; and a great amount of money, no longer wasted on them, would be available for other purposes.

THE VALUE OF SCIENCE

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Chapter III. The Notion of Space

1. *Introduction*

In the articles I have heretofore devoted to space I have above all emphasized the problems raised by non-Euclidean geometry, while leaving almost completely aside other questions more difficult of approach, such as those which pertain to the number of dimensions. All the geometries I considered had thus a common basis, that tridimensional continuum which was the same for all and which differentiated itself only by the figures one drew in it or when one aspired to measure it.

In this continuum, primitively amorphous, we may imagine a network of lines and surfaces, we may then convene to regard the meshes of this net as equal to one another, and it is only after this convention that this continuum, become measurable, becomes Euclidean or non-Euclidean space. From this amorphous continuum can therefore arise indifferently one or the other of the two spaces, just as on a blank sheet of paper may be traced indifferently a straight or a circle.

In space we know rectilinear triangles the sum of whose angles is equal to two right angles; but equally we know curvilinear triangles the sum of whose angles is less than two right angles. The existence of the one sort is not more doubtful than that of the other. To give the name of straights to the sides of the first is to adopt Euclidean geometry; to give the name of straights to the sides of the latter is to adopt the non-Euclidean geometry. So that to ask what geometry it is proper to adopt is to ask, to what line is it proper to give the name straight?

It is evident that experiment can not settle such a question; one would not ask, for instance, experiment to decide whether I should call AB or CD a straight. On the other hand, neither can I say that I have not the right to give the name of straights to the sides of non-Euclidean triangles because they are not in conformity with the eternal idea of straight which I have by intuition. I grant, indeed, that I have the intuitive idea of the side of the Euclidean triangle, but I have

equally the intuitive idea of the side of the non-Euclidean triangle. Why should I have the right to apply the name of straight to the first of these ideas and not to the second? Wherein does this syllable form an integrant part of this intuitive idea? Evidently when we say that the Euclidean straight is a *true* straight and that the non-Euclidean straight is not a true straight, we simply mean that the first intuitive idea corresponds to a *more noteworthy* object than the second. But how do we decide that this object is more noteworthy? This question I have investigated in 'Science and Hypothesis.'

It is here that we saw experience come in. If the Euclidean straight is more noteworthy than the non-Euclidean straight, it is so chiefly because it differs little from certain noteworthy natural objects from which the non-Euclidean straight differs greatly. But, it will be said, the definition of the non-Euclidean straight is artificial; if we for a moment adopt it, we shall see that two circles of different radius both receive the name of non-Euclidean straights, while of two circles of the same radius one can satisfy the definition without the other being able to satisfy it, and then if we transport one of these so-called straights without deforming it, it will cease to be a straight. But by what right do we consider as equal these two figures which the Euclidean geometers call two circles with the same radius? It is because by transporting one of them without deforming it we can make it coincide with the other. And why do we say this transportation is effected without deformation? It is impossible to give a good reason for it. Among all the motions conceivable, there are some of which the Euclidean geometers say that they are not accompanied by deformation; but there are others of which the non-Euclidean geometers would say that they are not accompanied by deformation. In the first, called Euclidean motions, the Euclidean straights remain Euclidean straights, and the non-Euclidean straights do not remain non-Euclidean straights; in the motions of the second sort, or non-Euclidean motions, the non-Euclidean straights remain non-Euclidean straights and the Euclidean straights do not remain Euclidean straights. It has, therefore, not been demonstrated that it was unreasonable to call straights the sides of non-Euclidean triangles; it has only been shown that that would be unreasonable if one continued to call the Euclidean motions motions without deformation; but it has at the same time been shown that it would be just as unreasonable to call straights the sides of Euclidean triangles if the non-Euclidean motions were called motions without deformation.

Now when we say that the Euclidean motions are the *true* motions without deformation, what do we mean? We simply mean that they are *more noteworthy* than the others. And why are they more note-

worthy? It is because certain noteworthy natural bodies, the solid bodies, undergo motions almost similar.

And then when we ask: Can one imagine non-Euclidean space? that means: Can we imagine a world where there would be noteworthy natural objects affecting almost the form of non-Euclidean straights, and noteworthy natural bodies frequently undergoing motions almost similar to the non-Euclidean motions? I have shown in 'Science and Hypothesis' that to this question we must answer yes.

It has often been observed that if all the bodies in the universe were dilated simultaneously and in the same proportion, we should have no means of perceiving it, since all our measuring instruments would grow at the same time as the objects themselves which they serve to measure. The world, after this dilatation, would continue on its course without anything apprising us of so considerable an event. In other words, two worlds similar to one another (understanding the word similitude in the sense of Euclid, Book VI.) would be absolutely indistinguishable. But more; worlds will be indistinguishable not only if they are equal or similar, that is, if we can pass from one to the other by changing the axes of coordinates, or by changing the scale to which lengths are referred; but they will still be indistinguishable if we can pass from one to the other by any 'point-transformation' whatever. I will explain my meaning. I suppose that to each point of one corresponds one point of the other and only one, and inversely; and besides that the coordinates of a point are continuous functions, *otherwise altogether arbitrary,* of the corresponding point. I suppose besides that to each object of the first world corresponds in the second an object of the same nature placed precisely at the corresponding point. I suppose finally that this correspondence fulfilled at the initial instant is maintained indefinitely. We should have no means of distinguishing these two worlds one from the other. The relativity of space is not ordinarily understood in so broad a sense; it is thus, however, that it would be proper to understand it.

If one of these universes is our Euclidean world, what its inhabitants will call straight will be our Euclidean straight; but what the inhabitants of the second world will call straight will be a curve which will have the same properties in relation to the world they inhabit and in relation to the motions that they will call motions without deformation. Their geometry will, therefore, be Euclidean geometry, but their straight will not be our Euclidean straight. It will be its transform by the point-transformation which carries over from our world to theirs. The straights of these men will not be our straights, but they will have among themselves the same relations as our straights to one another. It is in this sense I say their geometry will be ours. If then we wish

after all to proclaim that they deceive themselves, that their straight is not the true straight, if we still are unwilling to admit that such an affirmation has no meaning, at least we must confess that these people have no means whatever of recognizing their error.

2. *Qualitative Geometry*

All that is relatively easy to understand, and I have already so often repeated it that I think it needless to expatiate further on the matter. Euclidean space is not a form imposed upon our sensibility, since we can imagine non-Euclidean space; but the two spaces, Euclidean and non-Euclidean, have a common basis, that amorphous continuum of which I spoke in the beginning. From this continuum we can get either Euclidean space or Lobachevskian space, just as we can, by tracing upon it a proper graduation, transform an ungraduated thermometer into a Fahrenheit or a Réaumur thermometer.

And then comes a question: Is not this amorphous continuum that our analysis has allowed to survive a form imposed upon our sensibility? If so, we should have enlarged the prison in which this sensibility is confined, but it would always be a prison.

This continuum has a certain number of properties, exempt from all idea of measurement. The study of these properties is the object of a science which has been cultivated by many great geometers and in particular by Riemann and Betti and which has received the name of analysis situs. In this science abstraction is made of every quantitative idea and, for example, if we ascertain that on a line the point B is between the points A and C, we shall be content with this ascertainment and shall not trouble to know whether the line ABC is straight or curved, nor whether the length AB is equal to the length BC, or whether it is twice as great.

The theorems of analysis situs have, therefore, this peculiarity that they would remain true if the figures were copied by an inexpert draftsman who should grossly change all the proportions and replace the straights by lines more or less sinuous. In mathematical terms, they are not altered by any 'point-transformation' whatsoever. It has often been said that metric geometry was quantitative, while projective geometry was purely qualitative. That is not altogether true. The straight is still distinguished from other lines by properties which remain quantitative in some respects. The real qualitative geometry is, therefore, analysis situs.

The same questions which came up apropos of the truths of Euclidean geometry, come up anew apropos of the theorems of analysis situs. Are they obtainable by deductive reasoning? Are they disguised conventions? Are they experimental verities? Are they the

characteristics of a form imposed either upon our sensibility or upon our understanding?

I wish simply to observe that the last two solutions exclude each other. We can not admit at the same time that it is impossible to imagine space of four dimensions and that experience proves to us that space has three dimensions. The experimenter puts to nature a question: Is it this or that? and he can not put it without imagining the two terms of the alternative. If it were impossible to imagine one of these terms, it would be futile and besides impossible to consult experience. There is no need of observation to know that the hand of a watch is not marking the hour 15 on the dial, because we know beforehand that there are only 12, and we could not look at the mark 15 to see if the hand is there, because this mark does not exist.

Note likewise that in analysis situs the empiricists are disembarrassed of one of the gravest objections that can be leveled against them, of that which renders absolutely vain in advance all their efforts to apply their thesis to the verities of Euclidean geometry. These verities are rigorous and all experimentation can only be approximate. In analysis situs approximate experiments may suffice to give a rigorous theorem and, for instance, if it is seen that space can not have either two or less than two dimensions, nor four or more than four, we are certain that it has exactly three, since it could not have two and a half or three and a half.

Of all the theorems of analysis situs, the most important is that which is expressed in saying that space has three dimensions. This it is that we are about to consider, and we shall put the question in these terms: When we say that space has three dimensions, what do we mean?

3. *The Physical Continuum of Several Dimensions*

I have explained in 'Science and Hypothesis' whence we derive the notion of physical continuity and how that of mathematical continuity has arisen from it. It happens that we are capable of distinguishing two impressions one from the other, while each is indistinguishable from a third. Thus we can readily distinguish a weight of 12 grams from a weight of 10 grams, while a weight of 11 grams could neither be distinguished from the one nor the other. Such a statement, translated into symbols, may be written:

$$A = B,\ B = C,\ A < C.$$

This would be the formula of the physical continuum, as crude experience gives it to us, whence arises an intolerable contradiction that has been obviated by the introduction of the mathematical continuum. This is a scale of which the steps (commensurable or incommensurable numbers) are infinite in number, but are exterior to one

another instead of encroaching on one another as do the elements of the physical continuum, in conformity with the preceding formula.

The physical continuum is, so to speak, a nebula not resolved; the most perfect instruments could not attain to its resolution. Doubtless if we measured the weights with a good balance instead of judging them by the hand, we could distinguish the weight of 11 grams from those of 10 and 12 grams, and our formula would become:

$$A < B, \quad B < C, \quad A < C.$$

But we should always find between A and B and between B and C new elements D and E, such that

$$A = D, \quad D = B, \quad A < B; \quad B = E, \quad E = C, \quad B < C,$$

and the difficulty would only have receded and the nebula would always remain unresolved; the mind alone can resolve it and the mathematical continuum it is which is the nebula resolved into stars.

Yet up to this point we have not introduced the notion of the number of dimensions. What is meant when we say that a mathematical continuum or that a physical continuum has two or three dimensions?

First we must introduce the notion of cut, studying first physical continua. We have seen what characterizes the physical continuum. Each of the elements of this continuum consists of a manifold of impressions; and it may happen either that an element can not be discriminated from another element of the same continuum, if this new element corresponds to a manifold of impressions not sufficiently different, or, on the contrary, that the discrimination is possible; finally it may happen that two elements indistinguishable from a third, may, nevertheless, be distinguished one from the other.

That postulated, if A and B are two distinguishable elements of a continuum C, a series of elements may be found, $E_1, E_2, \cdots, E_n$, all belonging to this same continuum C and such that each of them is indistinguishable from the preceding, that E_1 is indistinguishable from A and E_n indistinguishable from B. Therefore we can go from A to B by a continuous route and without quitting C. If this condition is fulfilled for any two elements A and B of the continuum C, we may say that this continuum C is all in one piece. Now let us distinguish certain of the elements of C which may either be all distinguishable from one another, or themselves form one or several continua. The assemblage of the elements thus chosen arbitrarily among all those of C will form what I shall call the *cut* or the *cuts*.

Take on C any two elements A and B. Either we can also find a series of elements $E_1, E_2, \cdots, E_n$, such: (1) that they all belong to C; (2) that each of them is indistinguishable from the following, E_1 indistinguishable from A and E^n from B; (3) *and besides that none*

of the elements E is indistinguishable from any element of the cut. Or else, on the contrary, in each of the series E_1, E_2, $\cdots$, E_n satisfying the first two conditions, there will be an element E indistinguishable from one of the elements of the cut. In the first case we can go from A to B by a continuous route without quitting C and *without meeting the cuts;* in the second case that is impossible.

If then for any two elements A and B of the continuum C, it is always the first case which presents itself, we shall say that C remains all in one piece despite the cuts.

Thus, if we choose the cuts in a certain way, otherwise arbitrary, it may happen either that the continuum remains all in one piece or that it does not remain all in one piece; in this latter hypothesis we shall then say that it is *divided* by the cuts.

It will be noticed that all these definitions are constructed in setting out solely from this very simple fact, that two manifolds of impressions sometimes can be discriminated, sometimes can not be. That postulated, if, to *divide* a continuum, it suffices to consider as cuts a certain number of elements all distinguishable from one another, we say that this continuum *is of one dimension;* if, on the contrary, to divide a continuum, it is necessary to consider as cuts a system of elements themselves forming one or several continua, we shall say that this continuum is *of several dimensions.*

If to divide a continuum C, cuts forming one or several continua of one dimension suffice, we shall say that C is a continuum *of two dimensions;* if cuts suffice which form one or several continua of two dimensions at most, we shall say that C is a continuum *of three dimensions;* and so on.

To justify this definition it is proper to see whether it is in this way that geometers introduce the notion of three dimensions at the beginning of their works. Now, what do we see? Usually they begin by defining surfaces as the boundaries of solids or pieces of space, lines as the boundaries of surfaces, points as the boundaries of lines, and they affirm that the same procedure can not be pushed further.

This is just the idea given above: to divide space, cuts that are called surfaces are necessary; to divide surfaces, cuts that are called lines are necessary; to divide lines, cuts that are called points are necessary; we can go no further, the point can not be divided, so the point is not a continuum. Then lines which can be divided by cuts which are not continua will be continua of one dimension; surfaces which can be divided by continuous cuts of one dimension will be continua of two dimensions; finally space which can be divided by continuous cuts of two dimensions will be a continuum of three dimensions.

Thus the definition I have just given does not differ essentially from the usual definitions; I have only endeavored to give it a form applicable not to the mathematical continuum, but to the physical continuum, which alone is susceptible of representation, and yet to retain all its precision. Moreover, we see that this definition applies not alone to space; that in all which falls under our senses we find the characteristics of the physical continuum, which would allow of the same classification; that it would be easy to find there examples of continua of four, of five, dimensions, in the sense of the preceding definition; such examples occur of themselves to the mind.

I should explain finally, if I had the time, that this science, of which I spoke above and to which Riemann gave the name of analysis situs, teaches us to make distinctions among continua of the same number of dimensions and that the classification of these continua rests also on the consideration of cuts.

From this notion has arisen that of the mathematical continuum of several dimensions in the same way that the physical continuum of one dimension engendered the mathematical continuum of one dimension. The formula

$$A > C,\ A = B,\ B = C,$$

which summed up the data of crude experience, implied an intolerable contradiction. To get free from it it was necessary to introduce a new notion while still respecting the essential characteristics of the physical continuum of several dimensions. The mathematical continuum of one dimension admitted of a scale whose divisions, infinite in number, corresponded to the different values, commensurable or not, of one same magnitude. To have the mathematical continuum of n dimensions, it will suffice to take n like scales whose divisions correspond to different values of n independent magnitudes called coordinates. We thus shall have an image of the physical continuum of n dimensions, and this image will be as faithful as it can be after the determination not to allow the contradiction of which I spoke above.

4. *The Notion of Point*

It seems now that the question we put to ourselves at the start is answered. When we say that space has three dimensions, it will be said, we mean that the manifold of points of space satisfies the definition we have just given of the physical continuum of three dimensions. To be content with that would be to suppose that we know what is the manifold of points of space, or even one point of space.

Now that is not as simple as one might think. Every one believes he knows what a point is, and it is just because we know it too well that we think there is no need of defining it. Surely we can not be

required to know how to define it, because in going back from definition to definition a time must come when we must stop. But at what moment should we stop?

We shall stop first when we reach an object which falls under our senses or that we can represent to ourselves; definition then will become useless; we do not define the sheep to a child; we say to him: *See* the sheep.

So, then, we should ask ourselves if it is possible to represent to ourselves a point of space. Those who answer yes do not reflect that they represent to themselves in reality a white spot made with the chalk on a blackboard or a black spot made with a pen on white paper, and that they can represent to themselves only an object or rather the impressions that this object made on their senses.

When they try to represent to themselves a point, they represent the impressions that very little objects made them feel. It is needless to add that two different objects, though both very little, may produce extremely different impressions, but I shall not dwell on this difficulty, which would still require some discussion.

But it is not a question of that; it does not suffice to represent *one* point, it is necessary to represent *a certain* point and to have the means of distinguishing it from an *other* point. And in fact, that we may be able to apply to a continuum the rule I have above expounded and by which one may recognize the number of its dimensions, we must rely upon the fact that two elements of this continuum sometimes can and sometimes can not be distinguished. It is necessary therefore that we should in certain cases know how to represent to ourselves *a specific* element and to distinguish it from an *other* element.

The question is to know whether the point that I represented to myself an hour ago is the same as this that I now represent to myself, or whether it is a different point. In other words, how do we know whether the point occupied by the object A at the instant α is the same as the point occupied by the object B at the instant β, or still better, what this means?

I am seated in my room; an object is placed on my table; during a second I do not move, no one touches the object. I am tempted to say that the point A which this object occupied at the beginning of this second is identical with the point B which it occupies at its end. Not at all; from the point A to the point B is 30 kilometers, because the object has been carried along in the motion of the earth. We can not know whether an object, be it large or small, has not changed its absolute position in space, and not only can we not affirm it, but this affirmation has no meaning and in any case can not correspond to any representation.

But then we may ask ourselves if the relative position of an object with regard to other objects has changed or not, and first whether the relative position of this object with regard to our body has changed. If the impressions this object makes upon us have not changed, we shall be inclined to judge that neither has this relative position changed; if they have changed, we shall judge that this object has changed either in state or in relative position. It remains to decide which of the *two*. I have explained in 'Science and Hypothesis' how we have been led to distinguish the changes of position. Moreover, I shall return to that further on. We come to know, therefore, whether the relative position of an object with regard to our body has or has not remained the same.

If now we see that two objects have retained their relative position with regard to our body, we conclude that the relative position of these two objects with regard to one another has not changed; but we reach this conclusion only by indirect reasoning. The only thing that we know directly is the relative position of the objects with regard to our body. *A fortiori* it is only by indirect reasoning that we think we know (and, moreover, this belief is delusive) whether the absolute position of the object has changed.

In a word, the system of coordinate axes to which we naturally refer all exterior objects is a system of axes invariably bound to our body, and carried around with us.

It is impossible to represent to oneself absolute space; when I try to represent to myself simultaneously objects and myself in motion in absolute space, in reality I represent to myself my own self motionless and seeing move around me different objects and a man that is exterior to me, but that I convene to call me.

Will the difficulty be solved if we agree to refer everything to these axes bound to our body? Shall we know then what is a point thus defined by its relative position with regard to ourselves? Many persons will answer yes and will say that they 'localize' exterior objects.

What does this mean? To localize an object simply means to represent to oneself the movements that would be necessary to reach it. I will explain myself. It is not a question of representing the movements themselves in space, but solely of representing to oneself the muscular sensations which accompany these movements and which do not presuppose the preexistence of the notion of space.

If we suppose two different objects which successively occupy the same relative position with regard to ourselves, the impressions that these two objects make upon us will be very different; if we localize them at the same point, this is simply because it is necessary to make

the same movements to reach them; apart from that, one can not just see what they could have in common.

But, given an object, we can conceive many different series of movements which equally enable us to reach it. If then we represent to ourselves a point by representing to ourselves the series of muscular sensations which accompany the movements which enable us to reach this point, there will be many ways entirely different of representing to oneself the same point. If one is not satisfied with this solution, but wishes, for instance, to bring in the visual sensations along with the muscular sensations, there will be one or two more ways of representing to oneself this same point and the difficulty will only be increased. In any case the following question comes up: Why do we think that all these representations so different from one another still represent the same point?

Another remark: I have just said that it is to our own body that we naturally refer exterior objects; that we carry about everywhere with us a system of axes to which we refer all the points of space, and that this system of axes seems to be invariably bound to our body. It should be noticed that rigorously we could not speak of axes invariably bound to the body unless the different parts of this body were themselves invariably bound to one another. As this is not the case, we ought, before referring exterior objects to these fictitious axes, to suppose our body brought back to the initial attitude.

(To be continued.)

PATHOGENIC PROTOZOA[1]

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AT the present time the importance of protozoa-study is recognized in all branches of biological science where, as single-celled organisms, they illustrate the manifold principles of living things. Thus it is in physiology, in cellular biology, in psychology and in general biology. There is one field, however, a field that is daily growing more extensive, in which the importance of protozoa has only recently been recognized, and this is the field of pathology. To the medical world for the most part, the group of protozoa consists of the few types of parasitic forms that cause human disease, and in this world any one who has a knowledge of *Trypanosoma,* or *Amœba,* or *Plasmodium,* is a student of the *protozoa,* while a deeper knowledge makes him a biologist. At the present time there are many students of the group in this sense, and the relations of protozoa to human welfare bid fair to be the most popular aspect of protozoan study, while in the public mind already the term protozoa is apparently the synonym of some new and fearsome thing. Commissions for the study of protozoan diseases have been appointed in many countries and chairs for the study of protozoology have been established in the universities of Cambridge and London mainly for the study of the pathogenic forms of these unicellular animals. The present paper deals with a few aspects of this more recent field of protozoa work.

Malaria

Few pathologists in good standing gave a thought to protozoa until after the malarial organisms had been worked out and the life history completely known through the researches of biologists and surgeons. Thanks to the work of Laveran, Ross, Grassi and Schaudinn, there is no longer a phase in this disease that is unknown, and the relations of the various symptoms of the malady to stages in the life history of the organisms are perfectly established. I do not need to go into the malaria problem, for the life history of the organisms, their relations to the mosquito *Anopheles,* the coincidence of merozoite 'spore' formation and pyrexial attacks of the disease, are familiar to all who have followed, even remotely, the progress of medical science. I will pass

[1] A lecture delivered at Woods Hole, July 10, 1906.

on rather to less familiar problems that are to-day puzzling the medical world.

Amœbic Dysentery

Since 1860 various forms of *Amœba* have been found in the human intestine. In 1876 Loesch first claimed these rhizopods to be the cause of dysentery, and since that time students of the subject have been about equally divided into supporters of Loesch and his opponents. In recent times, however, the belief is widespread that two forms of the disease occur, one of which, tropical, pernicious, or amœbic dysentery, is invariably accompanied by the rhizopod *Entamœba histolytica,* which Schaudinn distinguishes from the ordinary harmless intestinal amœba *A. coli.* This form becomes a tissue and cell-infecting parasite and to this characteristic it owes its malignancy. Musgrave in the government laboratory at Manila has worked out the organism most carefully and has been successful in producing the disease in monkeys and other animals through cultures free from the ordinary forms of bacteria. The life history of the *Amœba* has been followed by Schaudinn, and in the present place this is perhaps more interesting than the pathological details.

The *Amœba* has little structural detail, a nucleated cell with minute form changes, a slight differentiation into ectoplasm and entoplasm, a great power of reproduction, by division and by budding, leading to masses of the organisms in the intestine and attached organs, where intestinal lesions, liver abscesses and the like, become the characteristic features of the disease. One interesting phenomenon worked out by Schaudinn is the preliminary nuclear metamorphosis before spore formation. In the majority of rhizopods in which the life history is known the formation of conjugating gametes is preceded by fragmentation of the nucleus either by division, or by a kind of nuclear secretion of chromatin. A condition of the cell, known as that of the 'distributed nucleus,' results from this fragmentation and each fragment of chromatin becomes the nucleus of one of the gametes. Unfortunately, Schaudinn gives no figures in his preliminary publication on *Entamœba,* but his description tallies exactly with analogous processes in other rhizopods, notably in *Arcella, Centropyxis, Difflugia* and others.

In *Entamœba* as in these free-living rhizopods the spore-like bodies resulting from this distributed condition of the nucleus conjugate and so bring about a renewal of vitality of the parasite, favorable to the infection of new hosts. Musgrave has shown that these parasites may live a free life in ordinary drinking water and that infection takes place presumably in this way, and his observations indicate that *Entamœba* is a facultative rather than an obligatory parasite of man.

Schaudinn regards *Entamœba coli* as a harmless commensal, but *Entamœba histolytica* as a definite tissue-destroying parasite, a faculty which gives to this form of tropical dysentery its pernicious and often fatal characteristic.

Rabies Furiosa

From dysentery to hydrophobia seems a long jump and yet we may find that these two diseases and with them others apparently as diverse, as small-pox, sleeping sickness, etc., owe their malignancy to the destructive action of protozoa on different tissues of the body. I take up rabies next because the organism of hydrophobia agrees in many of its characteristics with that of dysentery, and belongs, as I believe, on the present evidence, in the group of rhizopods.

Peculiar structures have been known for several years in the brain and nerve cells of animals with rabies. Negri, however, in 1904 was the first to suggest that these are as distinctive of hydrophobia as Plasmodium is of malaria, and he and his assistants succeeded in establishing the conviction that these 'Negri' bodies are sufficiently characteristic to afford a quick and safe diagnosis of rabies. Beyond the suggestion that these bodies are organisms comparable to the small-pox organism as described by Councilman and his collaborators, Negri did not attempt to outline their systematic position. From the apparent absence of nuclei and other structures in the Negri bodies, pathologists have been inclined to interpret them as secretions, or degenerations of a specific type, and very few have been hardy enough to regard them as protozoa. Among these few, however, is Dr. Anna W. Williams, of New York, who spent a summer at Woods Hole three years ago, working on protozoa, and who has done a great deal on pathogenic protozoa; and to her belongs the credit of establishing the protozoon nature of these enigmatical structures.

One of the curious features of pathogenic protozoa is that they are very refractory to the ordinary cytological stains, and do not behave like other cells under hæmatoxylin or methylin blue, etc. This is why the organism of syphilis was overlooked until last year, and this is why Negri failed to get any evidence of cell structure in his rabies organism. In all of his preparations and figures the bodies appear as highly vacuolated structures with no sign of nucleus or differentiated cell body. Dr. Williams, using the Giemsa stain, a method that has been singularly successful in staining delicate pathogenic protozoa, showed that not only do these Negri bodies have a definite structure with nucleus and cytoplasm, but also that a series of nuclear metamorphoses occur which are identical in their consecutive phases with the typical free-living rhizopods. Nuclear fragmentation takes place until as in *Entamœba* or *Centropyxis* or *Amœba,* etc., the cell is filled with chro-

matin particles. Dr. Williams also showed that reproduction by division and by budding takes place, and she failed only to make out the conjugation processes. From my own study of Dr. Williams's preparations, I have no doubt at all that she is right about the organism, and believe that, under her name *Neurorcytes,* it should be grouped with the rhizopod-like protozoa.

This work on *Neurorcytes* is also very interesting from the side light it throws on the small-pox problem. Here again, in certain phases of variola, we find curious intra-cellular and intra-nuclear bodies having a striking resemblance to the ordinary unstained forms of the Negri bodies. These small-pox bodies were early recognized as characteristic of this disease, and Guarnieri in 1892, believing they were protozoa of a specific kind, named the organism *Cytorcytes variolæ.* Perhaps the majority of pathologists to-day and many biologists, are opposed to this interpretation, and these bodies, like the Negri bodies, are more commonly regarded as specific secretions or degenerations than as protozoa. I have no doubt myself, from long study of these small-pox organisms, that they are protozoa, and believe that with fresh material and by using the stain which Dr. Williams has so successfully used for *Neurorcytes,* the last doubter will be convinced.

At the risk of going somewhat far afield in pathological speculation, let me briefly call attention to one other possibility of amœboid organisms and disease, *viz.,* cancer. For years it has been known that vegetable cells in ordinary edible forms, like the cabbage or turnip, etc., may be stimulated to abnormal multiplication by rhizopod parasites. Such a parasite—*Plasmodiophora brassicæ*—enters the young root cells of a cabbage, stimulates those cells to an unwonted degree of multiplication until great tumors are formed giving rise to the vegetable disease known as 'club-root.' The plant cells become storage reservoirs of the spores of *Plasmodiophora,* and when the plant dies down the spores are liberated in the soil. Now it has been argued that if vegetable cells can be stimulated to abnormal activity, there is no real biological objection to animal cells being similarly stimulated to division by animal parasites, and some pathologists have gone so far even as to see in certain cell inclusions of cancer peculiar bodies which they compared with the *Plasmodiophora* spores. The comparison, however, can not be sustained, and without going into the subject extensively I may say in short that nothing has ever been seen in cancer cells that can be interpreted as a protozoon parasite. This, however, does not weigh against the parasite theory of cancer—a theory which I personally, believe to be the only one that satisfactorily explains the disease. The organism of yellow fever has never been seen, but no one doubts the parasitic nature of that disease and the fact that the virus or the germs of yellow fever pass through the finest Berkefeldt

and Chamberland filters indicates that we have to do in this disease with organisms too minute to be seen. Ultra-microscopic protozoa are not the only ones which will do this, for small amœbæ may, under pressure, be forced through some of these filters, while in a few organisms, notably the flagellates, there are some phases in the life history when the individuals become so small that they are no longer discernible with the highest powers of the microscope. This is the case in certain of the trypanosomes and spirochætes, which are now known to cause some of the most malignant of human diseases.

Trypanosomiasis and the 'Sleeping Sickness'

In this country, and indeed in temperate zones generally, there is no dread of trypanosomes, and 'sleeping sickness' is more often the subject of thoughtless jest than of intelligent consideration. In England, where African interests are more keenly followed, a deep interest is taken in this matter, and the Liverpool School of Tropical Medicine deals largely with trypanosomiasis. In his presidential address before the British Association for the Advancement of Science, meeting in South Africa last summer, Colonel Bruce said:

> **Trypanosomiasis in man, the 'sleeping sickness,' which occurs on the west coast of Africa, particularly in the basin of the Congo, has within the last few years spread eastward into Uganda, has already swept off some hundreds of thousands of victims, is spreading down the Nile, has spread all round the shores of Lake Victoria and is still spreading southward round lakes Albert and Albert Edward, and now threatens the Transvaal and Zululand.[3]**

Many different species of trypanosomes are known, and the normal habitat is the blood. No form of vertebrate is exempt, the blood of fish and amphibia, reptiles, birds and mammals forming suitable culture media for their growth and reproduction. In some stages of their life history they apparently become intra-cellular parasites, lose their flagella and membranes and assume the gregarine-like form. In man they may be either comparatively harmless flagellates, swimming about in the blood plasm, or, by bursting of the capillaries, they may penetrate the membranes of the brain and spinal cord and give rise to the invariably fatal disease of man—'sleeping sickness.' The presence of *Trypanosoma gambiense* in the human blood gives rise to the 'trypanosome fever' of Africa, not much worse apparently than is malaria in this country, but when the parasites enter the nervous system and congregate in the cerebro-spinal fluid, or in the ventricles of the brain, the result is invariably fatal. The result of this nerve-invasion is the appearance of various nervous symptoms like apathy, lassitude, trembling, and, finally, somnolence, increasing to a phase of intense coma and

[3] *Science*, XXII., p. 298, 1905.

death in from three to twelve months. Death usually is due to the disorganization of the nervous system, but in chronic cases it may follow from weakness and emaciation, or, in acute cases, it may result from the blocking of the capillaries by the parasites.

The full life-history of the organism of sleeping sickness is not yet known. In all blood-dwelling protozoa, however, the infection is carried from individual to individual by some intermediate host, usually a blood-sucking invertebrate. *Trypanosoma gambiense* is thus transmitted from man to man by the tsetse fly, *Glossina palpalis; Trypanosoma brucei,* the 'tsetse fly disease' of horses, by *Glossina morsitans; Trypanosoma lewisi* is carried from rat to rat by the louse, *Hæmatopinus; T. ziemanni* from owl to owl by the mosquito, *Culex pipiens.* Nor are the intermediate hosts limited to the insects. *Trypanoplasma borreli,* for example, is transmitted from carp to carp by the fish leech, *Piscicola geometra.*

The history of the trypanosomes in the digestive tracts of these various carriers has been made out in several cases, although by no means in all. In the gut of the leech, of the louse, *Hæmatopinus,* and of the mosquito, three different species of *Trypanosoma* have been worked out by different and competent observers, and in all cases it has been found that this environment is the scene of the most important phases in the life history of the parasite, *viz.,* the conjugation stages, which lead to renewal of vitality. The flagellate parasites thus agree in the main features of their life cycles with the malaria organisms in man and in the insect host *Anopheles.* Indeed, so widespread is this phenomenon that we are justified in assuming, where actual evidence is not forthcoming, that similar important processes take place in all intermediate hosts, and that we must look for conjugation phases of the parasite of Texas fever (cattle) in the tick (*Boophilus bovis*), and of sleeping sickness in the fly (*Glossina palpalis*). On the other hand, we are justified in assuming a protozoon parasite, even though the parasite is not known, in cases where its existence has been proved in an intermediate host. This is the case, for example, in yellow fever, where a definite incubation period of the parasite of about twelve days in the mosquito *Stegomyia fasciata* is known, and where it is fully established that, apart from this mosquito, no other means of transmission of the disease exists.

Associated with the *Trypanosoma* diseases, although not yet established, are those curious maladies of India and similar countries, known as dum-dum fever, kala-azar, splenomegaly, etc. The infection may be general or localized, and curious structures, known as the Donovan-Leischman-Wright bodies, have been observed in the spleen of dum-dum fever patients, in the lesions in 'tropical ulcer,' Delhi boil, 'oriental sore' and the like, and these bodies have been interpreted as stages in

the history of some *Trypanosoma*. At the present time, however, it appears much more likely that they are more nearly related to the hæmosporidia than to the flagellates, and Laveran and Mesnil's view assigning them to the genus *Piroplasma*, the same genus as the parasite of Texas fever, may be accepted.

Diseases due to Spirochæta

Closely allied to *Trypanosoma* is the genus *Spirochæta*, a similar flagellate belonging to the order *Monadida*. In its general cork-screw shape it resembles *Spirillum*, one of the bacteria, but differs from this by reason of its plastic body and general mode of life. Many different species have been described, and considerable difference of opinion exists as to whether they should be classed as bacteria or as protozoa. One well-known form—*Spirochæta obermeieri*—has long been recognized as the cause of relapsing fever, and has been recently shown to be the cause of human tick fever in Africa, which is carried from man to man by the tick (*Ornithodorus moubata*). The organism of relapsing fever, however, does not seem to have a typical flagellate structure; there is unmistakable evidence of transverse rather than longitudinal division; its nucleus is distributed like that of a bacillus, and Novy has shown that it reacts like some bacteria during plasmolysis. The need of an intermediate host in the case of African tick fever seems to be the one distinctive protozoan characteristic.

While *Spirochæta obermeieri* is perhaps a doubtful protozoon, there is less doubt in the case of other species, some of which have unquestioned flagellate characters, including typical nuclear structures, longitudinal division and the like, while in a number of species an undulating membrane analogous to that of a trypanosome can be made out. Some of the better known species are: *Spirochæta dentium* of the mouth; *S. refringens* of ulcerating tumors; *S. gallinorum* a blood parasite of fowls, and *S. anodontæ*, parasitic in the crystalline style of the mussel *Anodonta mutabilis*. A similar parasite, described first as *S. balbiani*, is found in the crystalline style of the oyster, but it has so many trypanosome characteristics that it is now called *Trypanosoma balbiani*. It serves to illustrate the close relationship between these two genera.

It is to this group of parasites that *Spirochæta* (*Treponema*) *pallida*, the cause of syphilis, belongs. The organism was discovered a year ago, and since that time has been submitted to the widest range of pathological research. A full life history has been published by Siedlecki and Krzyszakowicz, and some of the stages described by them are strikingly similar to those of *Trypanosoma*. Schaudinn regards it as sufficiently distinct from other *Spirochæta* species to justify a new generic name and calls it *Treponema pallidum*.

The organisms causing the several diseases that I have dealt with in the preceding account fall in three of the four great groups of protozoa, the *Infusoria* alone being unrepresented. The causes of tropical dysentery, of rabies, and possibly of small-pox, are related to the ordinary rhizopod types; the causes of sleeping sickness, trypanosomiasis and syphilis belong to the animal flagellates, and here, possibly, belongs the organism of yellow fever; the causes of malaria, and, probably, of splenomegaly, dum-dum fever and the like belong to the sporozoa.

The effects of these different parasites upon their hosts differ in different cases. Sometimes they poison the host by the liberation of toxins, as in the case of malaria or yellow fever; sometimes by local lesions or general tissue disorganization, as in tropical ulcer, sleeping sickness, syphilis and rabies; or sometimes by the mere mechanical obstruction to normal physiological processes by the accumulation of parasites in capillaries and ducts, while in still other cases the parasites stimulate the latent dividing energy of tissue cells and lead to abnormal tumor-like growths. In many cases knowledge of the organism and of its mode of life has led to preventive measures and to the great saving of human life. In yellow fever, for example, the warfare on mosquitoes will stop entirely its epidemic nature and, thanks to Carroll's brilliant experiments, yellow fever to-day, like malaria or trichinosis, is an advertisement of ignorance or criminal negligence in communities where it exists. Preventive measures hold down the ravages of small-pox, anti-rabic serum lessens the malignancy of hydrophobia, while different specifics are fatal to other kinds of parasitic protozoa, quinine to *Plasmodium malariæ*, mercury to *Treponema pallidum*, and recently Koch's specific to *Trypanosoma*. With the knowledge of these other pathogenic protozoa and of their modes of life investigation will bring out the means of combating them and thus of reducing human suffering, and this prospect if for no other reason, is a full justification of the many commissions that have been appointed, and of the vast sums of money that have been spent, to further protozoan research.

THE MAKING OF THE GRAND CAÑON OF THE COLORADO

By Professor A. R. CROOK
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THE Grand Cañon of the Colorado furnishes the most impressive illustration of cañon-making forces to be found, and presents a fascinating chapter of world history.

Nearly every visitor to the Cañon attempts an explanation of the manner of its making, and their explanations may be broadly grouped under five types—the explanation of the Indians who for centuries have inhabited the Colorado River region, that of one of the oldest white settlers in Arizona, that of a successful Chicago business man, that of a famous literary man, and finally that of men who have studied various phases of world making long enough to have learned the chief principles involved.

The Indians who have lived in Arizona, generation after generation, might be expected to have some notion of the origin of the Cañon. Their explanation is that a wise chieftain long mourned the death of a beloved wife. Finally, in pity, one of the gods made a great cleft in the earth, took the chieftain to the Happy Hunting Ground to see his wife; and then upon their return, fearing that others might go that way, the god hurled a river in the cleft where it is now flowing, effectually barring the way against intruders. The 'god-made' theory of the Indians is unsatisfactory as the infant science of any race always is.

The explanation of one of the oldest settlers in the region is hardly better. It is that of the widely-known John Hance. One morning he entertained the writer with the picturesque stories which he has poured into the ears of many an amused traveler, telling how thirty years ago he went from the Atlantic coast to Arizona to find room for his energies. In the east he was crowded. In Arizona he found opportunity to expand. A few years ago he used to tell that he dug the cañon, but says that he no longer makes the statement, because a little girl asked him where he put the dirt! The man-made explanation of the white man is no more satisfactory than the god-made theory of the Indian.

A Chicago business man who knows how to make dollars, thinks he knows how the cañon was made, and after visiting it said to the author: "I'll tell you how that cañon was made. Once there was a

FIG. 1. ONE MILE FROM THE CAÑON—SEEMINGLY A THOUSAND.

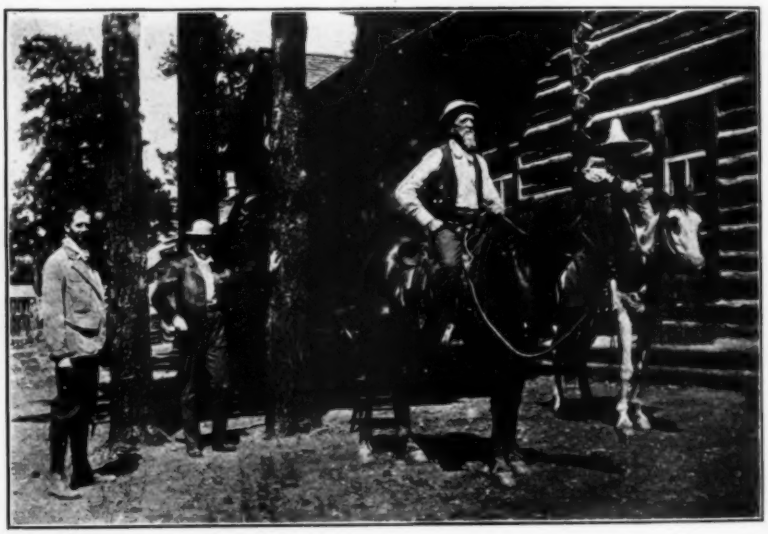

FIG. 2. JOHN HANCE AT OLD GRAND VIEW HOTEL.

gigantic volcano. It exploded with such fury that one side became the finest dust and the other toppled over and now forms the channel through which the river is flowing." Much nearer to the truth than any of these explanations is that of Joaquin Miller. He suggests that the cañon is due to an underground river which worked away like the Green River has done in Mammoth Cave. Finally the roof fell in and behold the Grand Cañon. A picturesque explanation is Miller's and better than any of the others. Its truth consists in representing the river as the active agent, its error in the underground idea.

The geologist knows that the Colorado River cut out the whole cañon and regards the region as the finest example of river work on a grand scale in the world and as the type of all that is gigantic in displacement and denudation. The manner in which the river accomplished its tremendous task can be more easily comprehended by considering a number of facts; first, such as relate to the geography of the region; second, to its topography; third, to its stratigraphy and petrology; fourth, to its denudation and displacements. These different groups are closely interwoven with each other and the facts marshaled in one group are dependent upon those in all of the other groups.

Many tourists returning from the state of Colorado report that they have visited the Grand Cañon of the Colorado when they have simply seen the cañon of the Arkansas River—a cañon which would be a mere scratch in the side of the Grand Cañon. To see the chief cañon in the world they should have journeyed several hundred miles farther towards the southwest, to the region where a cañon 200 miles long has cut off the northwest corner of Arizona from the remainder of that state. Most tourists will now make their visit at the point called Grand Cañon Post Office, while the more fortunate ones will go east to Grand View and west to Cataract Creek. The railroad from the main line has been built to a well-chosen point where the cañon is a dozen miles wide and where for fifty miles to the east and west it is most gigantic and impressive. Within this region it displays all the salient points in its topography, succession of geological formations, different kinds of rocks, and erosion and sculpturing. So that one inquiring about the origin of the cañon can obtain all the factors to the answer in this locality.

Thanks to the good topographic map which the government has issued, one does not have to guess at the distances and depths in the region. One may refer to the map and not be disappointed. Accurate measurements do not rob the depths and distances of their magnificence, nor is exaggeration there necessary for impressiveness.

The new survey shows that the elevation of the plateau through which the river has carved its path is between seven and eight thousand feet above sea level. At the head of the cañon the elevation of the river is twenty-five hundred feet, and by the time it has flowed two hundred miles to the end of the cañon it is only a thousand feet

FIG. 3. NEARLY FIVE THOUSAND FEET OF ROCK STRATA. OPPOSITE, A WALL A DOZEN MILES DISTANT.

above sea level. The distance between the top walls is from three to fifteen miles, and does not accord with the reports and pictures of the early government expeditions to the region. From El Tovar Hotel and the Grand View one sees the cañon at its broadest stretch, where for more than fifty miles it is a dozen miles wide and is filled with buttresses and buttes, towering from two to five thousand feet above the river.

The rim of the northern wall presents a nearly horizontal line. But a bird's-eye view from the western end of the cañon would show the country to the east to be divided into four plateaus, each dipping like cakes of ice in a river till met by its neighbor. The plateau north of the visitor at El Tovar Hotel is one of the blocks that has been raised highest and consequently has brought up the lowest rocks and exposed them to the corroding action of the river.

If with a high buzz-saw we could cut through the country from east to west for three hundred miles and could move out the section nearest us, we should see that the face of the cut opposite is composed of a series of blocks higher towards the west and slipping down towards the east. What we are unable to do in this regard has in a manner been done for us, since for ages the Colorado River has been eating away and has exposed the outlines of the blocks and the various geological formations so plainly that they are easily distinguished.

Seated on the rim in some favorable spot, one sees the different formations spread out as clearly as if diagrammed in a text-book on geology. The Kaibab plateau, just to the north shows six or seven

formations belonging to the four great rock systems. At the top is the bluish limestone which weathers into fantastic buttresses and pinnacles, many of them so dizzily perched as to give a constant challenge to the winds or to man to topple them from their precarious position and hurl them a thousand feet below. My companion could hardly find time to sleep, so great was his delight in prying off great blocks of stone and listening to their terrible crunch and roar as they fell hundreds of feet, striking a ledge here and there and finally crushed to powder, or, still in gigantic mass, they rested a thousand or more feet below. This top formation, which Powell called the 'Fortification limestone,' is the 'Upper Aubrey' limestone. It is about five hundred feet thick.

Next below it, can be seen the great white four hundred feet wide band of Upper Aubrey sandstone which stretches like a ribbon in sight for fifty miles and more to the east and west. Its walls are even more precipitous than those of the Aubrey limestone.

Below it are a thousand feet of shelving red sandstone which form the 'Lower Aubrey.' These three formations—the Upper Aubrey limestone and sandstone, and the Lower Aubrey sandstone—constitute the Upper Carboniferous system which is so familiar to the inhabitants of the Mississippi Valley states as the source of coal, but which in this region is not coal bearing.

Following the Upper Carboniferous a shelf of shales leads out to the precipitous wall of the great 'Red Wall' limestone, which, with

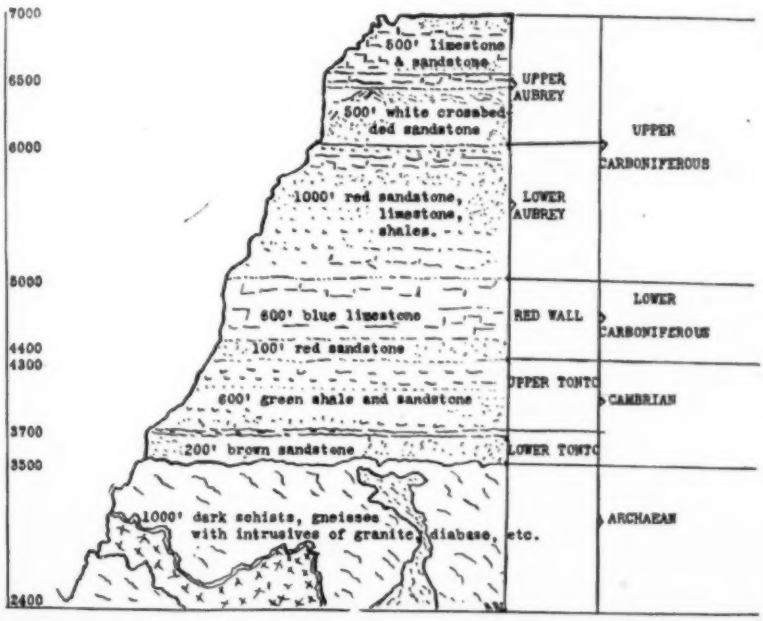

FIG. 4. GEOLOGICAL SECTION AT BRIGHT ANGEL TRAIL.

its six hundred feet of deep red cliffs, presents one of the most striking members of the landscape. The Red Wall constitutes the Lower Carboniferous of the region.

Then there is a terrace—'The Esplanade'—often several miles in width, composed of green shales and sandstones. It is the 'Upper Tonto' formation. It leads by gentle slopes to the brink of another precipice, which is formed by the quartzites and hard sandstones of the 'Lower Tonto.' The Upper and Lower Tonto are the local names of the formation composing the system known the world over as the Cambrian. Together they are about seven hundred feet thick. The Lower Tonto forms the capping of the inner cañon in the Bright Angel region.

FIG. 5. HURLING ROCKS OVER THE RIM. Upper Aubrey Limestones, Sandstone and Lower Aubrey Sandstone seen on Cliff two miles distant.

Below it are jagged, twisted, dark-colored metamorphic rocks, which represent the Archæan—the oldest system of rocks in the world. Tourists call these rocks 'granite,' but they are schists and gneisses. They are in layers and have been subjected to extensive folding. The river has cut through the Archæan rocks to a depth of over a thousand feet and produced walls so precipitous that in most places a sure-footed prospector could not cling to them. Between them the river runs with the greatest speed and fury.

The visitor may see all these formations from the upper rim, but as he descends to the river and passes them in succession, he obtains a clearer idea of their relations and characteristics. They are clearly differentiated by constitution, structure and color. A study of these various strata makes it evident that the topography of the country is

largely due to the difference in the kinds of rocks which the river encounters in its journey from the mountains to the sea.

Leaving the immediate vicinity of the cañon and going to the north, the tourist would come upon various younger geological formations which at one time buried the whole region. Although from ten to fifteen thousand feet thick, they no longer remain in the vicinity of the cañon, having been washed away by flowing water after the elevation of the land in early Eocene times. When that was accomplished and the country for miles around was comparatively level, the land was again gradually raised and the river began in Neocene times to cut the outer cañon. When it had cut down to the Tonto terrace—'The

FIG. 6. IN THE HEART OF THE CAÑON. Archaean Rocks capped by Tonto Strata, which dip towards the River.

Esplanade'—elevation ceased and the river wandered back and forth in a lazy manner, widening instead of deepening its channel. Since in that arid region there was not enough rainfall to wash down the side walls, they have remained precipitous, while the main stream has had time to push them back until they are a dozen miles apart. Then, in the Pliocene, slow elevation of the plateau began again, causing the river to run swiftly and renew its carving. This action is proceeding at the present time with the result of deepening the inner cañon.

Standing on the cañon's rim and looking down upon the immense gulf, one marvels that so small a river could have accomplished so gigantic a work. But if he laboriously descends to the brink of the roaring river below and puts his hand into the rushing water and feels

its burden of sharp sand driven along with great force, he obtains some conception of the efficiency of the tool. Quartz sand is harder than any of the common minerals in the rocks. Hurled by the rushing water against the sides and bed of the river, it cuts out a path through the rocks as a file does through soft iron.

Volcanic explosions had nothing to do with the making of the cañon. If they had, there would be volcanic rock over all the region instead of in isolated patches, and all the topographic forms would be different. The cañon is not the result of cracking apart of the earth's crust. If it were, the rock layers would dip away from the cañon; opposite sides would not match; they would lack marks of cutting parallel to the

FIG. 7. AT DAWN IN THE CAÑON.

bedding; their configuration would be independent of alternating hard and soft layers. The extensive cracking and faulting which does exist has been at right angles to the cañon and has presented different kinds of rocks for the river to work upon, thus producing variously shaped walls. As the river carves into succeeding strata the crust is weakened and various buttresses sink towards the river.

Thus all signs lead to the conclusion that fire did not make the cañon, nor did wind, nor earthquake, but that it was made by the same agent which in an hour carves tiny channels in a garden after a rain storm. That agent was running water, the water of the Colorado as for unnumbered years it has been flowing from snowy Rocky Mountain ridges to the hot sands which border the Gulf of California.

NOTES ON THE DEVELOPMENT OF TELEPHONE SERVICE

By FRED DE LAND
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I. *The Electric-Speaking Telephone*

THE desire to transmit speech over long distances probably dates back to the first wide separation of loved and beloved. That many methods have been suggested for the transmission of speech is of record. That speech has been mechanically or acoustically transmitted over many hundred feet of taut string and practically straight wires, during several hundred years, is true. That the 'lover's telephone' is a toy that has amused several generations is well known. That there were musical (not speech) telephones and sound (not speech) microphones nearly a century ago is an interesting fact. That prior to 1876, many men devoted much thought to the problem of the electrical transmission of speech is granted.

Nevertheless, the facts are that no authentic record has been found proving the existence of a practical method of speech transmission over long-distances, either electrical or mechanical, prior to the invention of the electric-speaking telephone by Alexander Graham Bell.

Moreover, while certain devices not invented by him are in use on telephone lines, each and all are but refinements or conveniences of the system. The broad fundamental method conceived by Alexander Graham Bell, in 1874, underlies the electrical transmission of speech in any form, and in any portion of the world. And Bell's method has been in no wise changed since its promulgation in letters-patent, in 1876, though thousands of the brightest minds in all civilized countries have striven for nearly thirty years to find another way, some other way, any other way, to transmit speech electrically.

Eighteen months passed by after the method conceived and perfected by Alexander Graham Bell became public property, and tens of thousands of Bell's telephones went into daily commercial use, before the first of the many claimants publicly asserted a prior right to the discovery of the art of transmitting speech electrically. Yet, not one could make his apparatus convey speech, except through a utilization of Bell's method and, in some cases, only by using Bell's receiver.

Thus the only reasonable conclusion that men in search of the truth can arrive at is 'that for physical and mathematical reasons it is not possible to have any method except the way that Alexander Graham Bell found.' And that was the sworn testimony unwillingly given by the experts employed by the followers in his footsteps. For the evi-

dence filed in a score of courts should satisfy all honest and unbiased minds that Bell's way is not only 'the only way' in which an electric-speaking telephone can transmit speech, but the absence of any earlier published description of a conception of a method similar in character to that first promulgated by Alexander Graham Bell proves that speech never was successfully transmitted electrically prior to its transmission by him.

After inventors, electricians and scientists had experimented with Bell's telephone for nearly two years, Dr. C. J. Blake told the eminent gentlemen who had gathered in London, in May, 1878, to hear his interesting lecture on the telephone:

> **When we consider the complex character of the waves resulting from the production of articulated sounds, and the loss in the excursions of the receiving disk (in the telephone), the wonder grows that this piece of metal can by its mechanical vibration reproduce so clearly and distinctly the delicate shades of quality of the human voice. That this should have been so perfectly accomplished is the result, not of inspiration, but of laborious research, and the instrument of which we reap the benefit to-day is the product, not merely of the genius, but of the patient and persistent labor of Alexander Graham Bell.**

In other words, the achievement of the seemingly impossible in the invention of the electric-speaking telephone came not through utilizing the idea or suggestion of another, nor in improving a philosophical or experimental instrument advertised and sold for many years prior to 1875, as a telephone; neither was it gained in a momentary inspiration, nor through automobilic rushes along the by-paths of superficial education.

This creation of a new art followed as the natural outcome of an original and magnificent conception that won the plaudits of scientists in all lands. The invention of the electric-speaking telephone (not the string telephone, nor the make-and-break musical telephone) followed in natural sequence. The combination of diaphragm and electro-magnet was the outgrowth of conception and perfected theory, and formed a practical materialization of both. And conception, theory and apparatus were the honestly earned fruits not only of the inventor's 'intellectual capacity and precision of thought,' but of a thorough knowledge of the essential elements in every factor entering into the problem of speech transmission; a knowledge gained through long and patient research, through many experiments, through financial expenditures that involved personal deprivations and hardships and necessitated the strictest self-denial, and through discouraging criticisms and bitter ridicule on the folly of wasting time and money in inventing 'a scientific toy.'

The magnitude of the masterly conception of creating, controlling and varying the strength or flow of the electric current by the spoken words, and making that current the vehicle for the transmission of the form or quality as well as the pitch and strength of the spoken words, and of delivering at a distant point the same words, with the

pitch, strength and quality unchanged; the comprehensive scope of the newly-created art of speech transmission; the remarkable transmitting qualities of the first of all electric-speaking telephones; and the promptness with which the inventor placed before the public a full and complete knowledge of his invention and of the essential steps leading to his application for letters-patent, all go to prove the possession in 1874–6 of an unusual knowledge on the part of Alexander Graham Bell, the more remarkable in view of the slight grasp electricians then possessed of magnetic action and the interrelation of the magnetic field and the electric current.

II. *The Telephone Exhibit at the Centennial Exposition*

In 1874, Alexander Graham Bell evolved his magnificent conception of the transmission of speech over long distances by means of the electric-speaking telephone. Theoretically it was perfect; practically it had no tangible existence. Men eminent in their respective professions, to whom he confided his plans in the autumn of 1874, admitted that while in theory the undulating-current method 'was adequate to the transmission of speech,' yet the electrical effect produced by the vibration of a diaphragm-armature actuated only by the human voice 'would be entirely too small to accomplish the desired end.' In fact, so complete was the absence of practical knowledge concerning the electrical effect that would be produced by causing the spoken word to vibrate an armature in front of an electro-magnet that the experts, most competent to pass upon the value of such an invention as the electric-speaking telephone, testified that the state of the art was such at the date of the patent that it could hardly have been supposed that a magneto-generator moved by a force so slight as the spoken word, 'would under any circumstances be able to generate an electric current which would produce upon a receiving instrument any effect whatever which would be perceptible to the senses.'

Discouraging though the advice and the suggestions of his friends proved, and disheartened though he was by ill-health and the lack of funds to carry on his telephone experiments, never did the inventor allow aught to divert his firm purpose of transforming that marvelous theory into a tangible speech-transmitting telephone.

In 1874, Alexander Graham Bell occupied the chair of vocal physiology in the Boston University, and supplemented his lectures 'by experimental demonstration of the practicability of correcting stammering, stuttering, lisping, burring and other defects of speech.' To a class composed exclusively of teachers of the deaf who had been sent to Boston by various institutions for the deaf throughout the United States, he delivered courses of lectures upon the subject of teaching articulation to deaf pupils, experimentally demonstrating his methods by giving instruction to deaf-mutes. He also had a class of

young women who desired to qualify as teachers of articulation in schools for the deaf. Then he had also undertaken the general education of a young deaf-mute child who came to him at the age of five years, in October, 1872.

Thus it was easy to understand how fully occupied the daylight hours were with his professional duties, and how any experimental and research work must necessarily be carried on late at night when most persons had retired. For while 'the income from his professional labors was a fairly good one it was his only means of support.' If he gave up his classes, he would have no income to expend upon experiments, nor even to live upon.

Yet that is what he did in 1875. At the end of the term he dismissed his classes and by the end of July, 1875, had given up all professional work save the education of the deaf lad and occasional lectures at the Boston University which he had long before been paid to deliver, and the income from which he had expended in telephone experiments. Thus the only income he was in receipt of at the end of July was for teaching the little deaf boy. He would not give up trying to solve the problem of speech transmission, and thus he borrowed what money generous friends would loan him, and started in to demonstrate the practicability of his theory.

All the world knows how well he succeeded, and that no better way has been found during all the years that intervene. In October, 1875, he commenced to prepare the specifications for the patent office, and had his application ready for filing before the end of the year. Then he waited on friends in Canada who desired him to take no action that would be prejudicial to patents they proposed taking out in foreign countries. These friends failing to respond to his proposition, he finally decided to wait no longer. In December he submitted his application to the patent attorneys in Washington; it was signed and sworn to in Boston on January 20, 1876, and filed in the Patent Office early on February 14. On March 7, 1876, he was granted the fundamental patent covering both method and apparatus.

Early in November, 1875, the need of funds to enable him to live forced Alexander Graham Bell to again take up his professional work, and he was soon 'lecturing at various normal schools upon the subject of articulation teaching.' A little later he established a large normal class in Boston, and to be able to properly illustrate his methods, gave free instruction to such deaf-mutes as would serve as subjects for demonstration. Thus by the spring of 1876 he was again in receipt of a fair income and began to repay the sums friends had loaned to him.

On May 10, 1876, at the solicitation of his friends, he read a paper, entitled 'Researches in Telephony,' to the members of the American Academy of Arts and Sciences. Therein he touched upon the inventions of Gray and Reis, and the discoveries of Page, Marrian, Beatson, Gassiot, De la Rive, Matteucci, Guillemin, Wertheim, Wartmann,

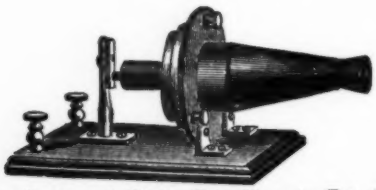
Janniar, Joule, Laborde, Legat, Poggendorff, DuMoncel, Delezenne, Ferguson, Paul la Cour, Helmholtz, Gore, Sullivan and others, giving due credit to each. Then he described his undulating current and his electric-speaking telephone, for which letters-patent had been granted, and made it clear to his hearers that the essential factor in this problem was not the devising of a definite form of instrument or tube, nor an apparatus having definite structural peculiarities, nor the combining of a certain number of parts into an operating whole. It was to so cause the electric current to flow that the receiver would not only reproduce a few or a majority or about all of the spoken words that impinged on the diaphragm of the transmitter in the form of sound waves, but would reproduce *each* and *all* and *every one* of the variations in the articulations, loudness and pitch and quality, with all their varying characteristics, whether expressed in the slightest whisper, in the soft voice of the cultured woman, in the sonorous rounded sentences of the dignified professor, or in the quick, abrupt remarks of the man of affairs.

And it may be added that the discovery and practical application of the method so described by which the changes in the current strength in a telephone diaphragm were brought about forms the essential and underlying principle of every successful electric-speaking telephone designed since Alexander Graham Bell created the art of telephony, a composite art, combining magnetism, electricity, acoustics, phonetics, mechanics and engineering.

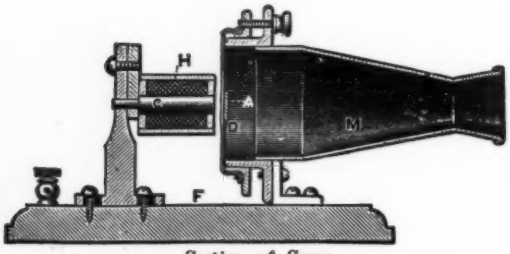
Gardiner Greene Hubbard was in charge of the Massachusetts educational exhibit at the Centennial Exposition. He insisted on placing the primitive telephones on exhibition in that section if no more suitable place could be secured. Alexander Graham Bell's time was too fully occupied with his professional work to give the subject any attention, and he really did not care whether the telephones were exhibited at the Centennial or not. Class examinations in his school were approaching, and he was far more interested in perfecting the knowledge of the members of his classes who were going forth to instruct deaf children in speech and speech-reading, than in a mere display of an invention that he had completed, patented and had described in a public lecture. An exhibit meant more or less outlay. He was still in debt to his friends, and the funds to repay his friends must come from the income from his school. Therefore the school would be taken care of to the exclusion of the Centennial.

Nevertheless, Mr. Hubbard secured some telephones and certain telegraphic instruments, and placed them on a table in the space allotted to the department of education and science of Massachusetts, which occupied a portion of the gallery at the east end of the main building. This modest display was labeled 'Telegraphic and Telephonic Apparatus. By A. Graham Bell.' It included his system of harmonic telegraphy, and his method of transmitting articulate speech

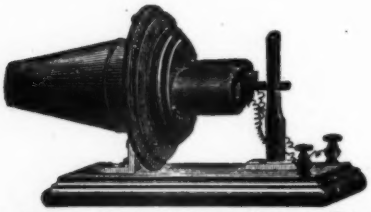
electrically. There were two membrane-diaphragm magneto-telephones, which could be used either as transmitters or as receivers. The only difference was that in one a single-pole electromagnet was used and a double-pole electromagnet in the other. The armature of each electromagnet consisted of a small piece of steel spring glued to the center of the membrane which was three inches in diameter. The castings were of brass, mounted on black-walnut base-boards, and the cones were of japanned tin (Fig. 1).[1] A third form of speaking tele-



Bell's Centennial Single Pole Magneto Telephone.



Section of Same.



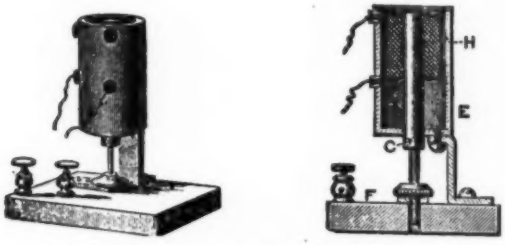
Bell's Centennial Double Pole Magneto Telephone.

FIG. 1.

phone was shown, intended to be used only as a receiving instrument. The electromagnet was enclosed in a hollow box of iron, and a lid of iron was used as an armature. This lid formed a thin circular metallic diaphragm, resting by its edge upon the rim of the iron box, its central portion not quite touching the pole of the electromagnet underneath. This receiver could be placed in circuit with either one of the mem-

[1] The illustrations shown (Figs. 1 to 5) are reproduced with permission from the general brief of the American Bell Telephone Company presented in 'The Telephone Appeals,' before the Supreme Court of the United States, October term, 1886.

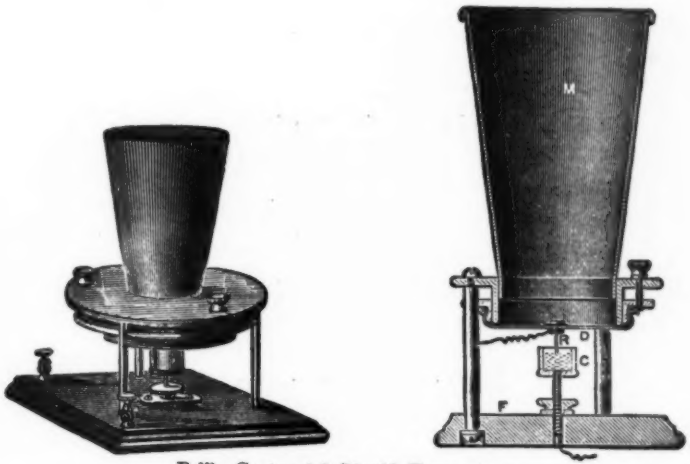
brane telephones. Upon placing the ear against the lid of the box the articulation of the person speaking into the membrane telephone was audible, proceeding from the lid (Fig. 2). A fourth form of



Bell's Centennial Iron Box Magnet Receiver.

FIG. 2.

speaking telephone was shown, intended to be used only as a transmitter. It consisted of a metallic ring supported horizontally, carrying a stretched membrane, to the center of which was fastened a platinum wire dipping into a little cup containing acidulated water and was referred to as the liquid transmitter (Fig. 3). In addition



Bell's Centennial Liquid Transmitter.

FIG. 3.

to these telephones, one of Koenig's manometric capsules was shown, arranged for studying optically the peculiarities of the electrical current generated by the movements of the membrane in the telephone.

Of telegraphic and signaling apparatus there was an excellent display at the Centennial, as well as a historical collection of great value showing the evolution of magnetic and electric signaling. But of other electrical apparatus there was only a meager display. All was included in Group XXV., Instruments of Precision.

Learning that the Centennial judges had arranged for a special inspection of certain telegraph and electrical apparatus on Sunday, June 25, when only those specially invited would be present and the building would be comparatively quiet, Mr. Hubbard sent a telegram requesting Graham Bell to arrive in Philadelphia not later than Sunday morning. But the class examinations were of far more importance just then than any explanation he could give the judges. So Graham Bell decided not to go. Then he received a message that Mr. Hubbard's daughter, Mabel (who a year later became Mrs. Bell), was leaving for Philadelphia, and hurried to the station in Boston to bid her good-bye. Mabel was as anxious as was her father to have Graham Bell explain his invention to the judges at the Centennial, and when she found that his sense of duty to his classes outweighed her influence, woman-like she burst into tears just as the train started. This was more than Graham Bell could stand, so he jumped aboard the moving train and with neither ticket nor baggage went through to Philadelphia.

As the weather was excessively hot in Philadelphia on Saturday evening Mr. Hubbard went to his Washington home 'tired out.' Sunday was another hot day, and after the judges had completed their inspection of all the other exhibits, and 'when it was already late, and the allotted time very nearly exhausted,' and while 'it was very warm, and signs of impatience were becoming manifest on the part of some of those present,' Graham Bell extended an invitation to the judges to inspect his telephones.

After some delay and discussion the judges proceeded to the Massachusetts section, where the apparatus was exhibited on a small table in a narrow space 'between the stairway and the wall.' Accompanying the judges were a large number of distinguished visitors, including the emperor of Brazil. Fortunately, but a short time before, the emperor had visited Graham Bell's school in Boston and had become interested in the method of instruction and also in the telephone. When Graham Bell saw that the emperor was with the judges he did not dream that his brief interview would be recalled. But his majesty cordially greeted him and spoke so enthusiastically about the telephone, that, tired as the judges were, they concluded to investigate thoroughly its merits. And from that moment the future of the telephone was assured.

Chief among the judges was Sir William Thomson (now Lord Kelvin), then and now the world's leading electrical scientist. He listened at the receiver, while Graham Bell's assistant talked into the transmitter. Sir William then went to the distant transmitter and repeated certain lines from Hamlet, which were heard as the receiver passed from one visitor to another. Forgotten was all thought of fatigue, of heat and discomfort, and nearly every visitor was glad of the opportunity of going to the end of the line and talking into that crude transmitter.

> The line extended from one part of the building to another, and the transmitting and receiving ends were sufficiently far apart to prevent the possibility of the speaker's voice being directly heard at the receiving end through the air.

Only the membrane magneto transmitter and the iron box receiver, were used. In recounting the enthusiasm that was aroused, Professor Barker said:

> I was greatly astonished and delighted to hear *for the first time* the transmission of articulate speech electrically. The mode of operation of the instrument was obvious at once, as soon as it was exhibited: it was one of those marvelously simple inventions that causes one to wonder, on seeing it for the first time, that it had not been invented long before.

At the close of the inspection Sir William Thomson expressed his regret that his wife had not been present to participate in such a marvelous experiment as the electrical transmission of speech, and asked Graham Bell if similar experiments could be enjoyed the following evening. Graham Bell replied that the apparatus was at the disposal of the judges and that they might experiment to their heart's content; but that he must be in Boston Monday morning in order to take care of the class examinations. To him, his school was of far more importance at that moment than 'the scientific toy' he had been chaffed about for many months. He left for Boston that evening and never returned to the Centennial.

That same evening the eminent English scientist, T. Sterry Hunt, wrote to Graham Bell:

> I am informed that you leave to-night for Boston, so I take this way of congratulating you on your success to-day. I returned to my hotel with Sir William Thomson, and dined with him. He speaks with much enthusiasm of your achievement. What yesterday he would have declared impossible he has to-day seen realized, and he declares it the most wonderful thing he has seen in America. You speak of it as an embryo invention, but to him it seems already complete: and he declares that, before long, friends will whisper their secrets over the electric wire. Your undulating current he declares a great and happy conception.

On the following day the telephones were removed to the judges' pavilion, and on that Monday evening many experiments were carried on. Wires were first run from a table in the private room of the special jury on instruments of precision to a table placed in the hallway near the main entrance. Owing to the thinness of the partitions, and the possibility of the loud-spoken words being heard over so short a distance, the wires were extended and the transmitter and table were carried some distance out-of-doors. Sir William Thomson and Lady Thomson were present and conversed with each other. Sentences were read from the New York *Tribune,* such as 'the American residents in London have decided to celebrate the Fourth of July,' and as each sentence was received Sir William Thomson would write it down in his note book and then go to the transmitting end of the line and compare what he had heard with what had been read. Most of the routine transmitting was done by Professor Watson, of Ann Arbor, whose voice

appeared to transmit most readily. "The particular instruments actually used were the 'membrane telephones' as transmitters and the 'membrane telephone' and 'iron box magneto receiver' as receivers." At the end of the week these instruments were replaced in the exhibit space in the gallery.

During that week thorough experiments were carried out and at their conclusion an award was made to Graham Bell by the judges of the group, while a special report drawn by Sir William Thomson, and a general report prepared by Professor Joseph Henry, secretary of the Smithsonian Institution and chairman of the judges, was published by the government.

Sir William Thomson said, in part:

> In addition to his electric-phonetic multiple telegraph, Mr. Graham Bell exhibits apparatus by which he has achieved a result of transcendent scientific interest —the transmission of spoken words by electric currents through a telegraph wire. To obtain this result, or even to make the first step towards it—the transmission of different qualities of sounds, such as the vowel sounds—Mr. Bell perceived that he must produce a variation of strength of current in the telegraph wire as nearly as may be in exact proportion to the velocity of a particle of air moved by the sound; and he invented a method of doing so—a piece of iron attached to a membrane, and thus moved to and fro in the neighborhood of an electro-magnet—which has proved perfectly successful. . . . This, perhaps *the greatest marvel hitherto achieved by the electric telegraph*, has been obtained by appliances of quite a homespun and rudimentary character. With somewhat more advanced plans, and more powerful apparatus, we may confidently expect that Mr. Bell will give us the means of making voice and spoken words audible through the electric wire to an ear hundreds of miles distant.

The chairman, Professor Joseph Henry, in his official report, said:

> The telephone of Mr. Bell aims at a still more *remarkable result*, that of transmitting audible speech through long telegraph lines. In the improved instrument the result is produced with striking effect, without the employment of an electrical current other than that produced by the mechanical action of the impulse of the breath as it issues from the lungs in producing articulate sounds. . . . Audible speech has in this way been transmitted to a distance of three hundred miles, perfectly intelligible to those who have become accustomed to the peculiarities of certain of the sounds. . . .

Another of the judges was Professor F. A. P. Barnard, president of Columbia College. A little later he publicly stated that

> Of all instruments of precision and research which the group of Centennial judges was called upon to examine, there was none that occasioned greater interest or that they regarded as of higher novelty and importance than the speaking telephone of Professor A. Graham Bell,

and he was confident

> that the name of *the inventor of the telephone* would be handed down to posterity with a permanent claim on the gratitude and remembrance of mankind.

Graham Bell was confident that he could transmit speech from Boston to Philadelphia, and, after his class examinations were over, he endeavored to secure the use of a telegraph circuit for that purpose, but failed because all 'were too busy.' Mr. Hubbard endeavored to

secure a circuit from Philadelphia, but was unsuccessful for the same reason.

On July 11, 1876, Graham Bell varied the shape of the iron armature by attaching to the membrane a thin disk of Tagger's iron, almost as large as the membrane. The next day he gave one of these telephones to Sir William Thomson. It is said that during the trip home the armature became bent, and useless in that condition. Nevertheless, Sir William used it to illustrate to the members of the British Association for the Advancement of Science how Graham Bell's telephone was the most marvelous of all the wonderful exhibits he had seen in America.

Graham Bell continued his experiments in improving the telephone, and finding that the large iron disk was far superior to the small armatures previously used, he concluded to dispense with the membrane altogether. This he did in October, 1876. On fastening a thin disk of steel in front of the electromagnet, conversation was carried on more easily than ever before. Referring to this improvement, Mr. Storrow said:

> Perhaps the most important contribution which Mr. Bell made towards improving speaking telephony, after the great conception and original instrument of his first patent, consisted in the wonderful sensitiveness and quickness of operation which he introduced into the instrument of the second patent, in consequence of the conviction which he reached by *study*, thought and experiment, that by so proportioning and combining all his parts as to sacrifice absolute strength to absolute quickness, he could obtain the best results; and then his innumerable experiments led him to the surprising discovery that a piece of sheet iron was much quicker and more faithful in following the delicate changes required for speech than the most delicate membrane is.

Nine years later the commissioner of patents, under date of March 3, 1885, wrote:

> Bell's patent was issued on the 17th of March, 1876. At the Centennial Exposition, held at Philadelphia that year (1876), he exhibited his telephone, and it was adjudged by such eminent scientists as Professor Henry, Sir William Thomson, of England, and Professor Gray, one of the contestants herein, to be a success, and the world recognized Bell as the first inventor of a speaking telephone. The indications are that it was not until the promised reward for so important a public service became visible that his claim of priority was called in question by any of the parties to this interference.
>
> That all the efforts of the several contestants who had attempted to produce a speaking telephone were failures seems clear from the record: that Bell was the first to give to the world the art of transmitting articulate speech, and the apparatus by which it could be successfully practised, was substantially conceded for a long period after his success in that behalf was placed beyond doubt. Whether or not these several contestants had the instrumentalities and appliances at that time from which success might have been realized if those instrumentalities had been better understood is of little consequence. The history of their experiments is a history of recorded failures.

III. *Devising the Telephone Exchange System*

Thirty years ago this summer the annual meeting of the British Association for the Advancement of Science was held in Glasgow,

Scotland. On that occasion men eminent in their respective professions listened with the deepest interest, while the president, Sir William Thomson (now Lord Kelvin), gave a vivid description of his visit to the Centennial, and stated that the most marvelous of all the wonderful exhibits he had seen in America was a pair of rudely-constructed telephones!

Then he explained to the members how surprising it all seemed when on that memorable Sabbath in June, 1876, to his listening ear came the words spoken at the distant end of the line; and he added:

> **All this my own ears heard, spoken to me with unmistakable distinctness by this circular disk armature of just such another little electro-magnet as this which I hold in my hand. . . . This, the greatest by far of all marvels of the electric telegraph, is due to a young countryman of our own, Mr. Graham Bell, now becoming a naturalized citizen of the United States. Who can but admire the hardihood of the invention which devised such a very slight means to realize the mathematical conception that, if electricity is to convey all the delicacies of quality which distinguish articulate speech, the strength of this current must vary continuously and as nearly as may be in simple proportion to the velocity of a particle of air engaged in constituting the sound?**

Sir William Thomson was then and is now the leading electrician of the world. And it was this generous endorsement of Alexander Graham Bell's invention that brought the telephone to the attention of scientific bodies in all countries, and led learned men in all lands to investigate its merits and to strive to improve its technical value. For in its remarkable simplicity the invention was a disappointment to many men, until practical experience demonstrated that the more elaborate copies were no more serviceable as speech-transmitting devices than the primitive original instrument. Nor during all the intervening years that have elapsed since 1876 has any inventor or any mechanician or any scientist ever suggested a more complete or a simpler description of the conception of the electric-speaking telephone and its governing principles than Graham Bell embodied in his application for a patent.

Yet that simple invention has exerted a far more potent influence than any of the more attractive fruits of inventive genius in revolutionizing and enriching custom and method in almost every branch of industry, of commerce and of society. And no other invention has so marvelously increased the scope of human usefulness and intelligent activity. With its aid time and distance are virtually eliminated, and Maine and Missouri and Mississippi and Minnesota are distant from each other only the length of a telephone call.

Yet marvelous as was the achievement of inventing the electric-speaking telephone, equally meritorious was the breadth of mind that could entertain at a time when poverty was pressing a prophetic vision of one vast transcontinental telephone system uniting every important village, town and city with wire highways over which messages would speed as quickly as thoughts are spoken.

Had the telephone been sold outright, in place of being leased for use in designated territory, it is very doubtful if a transcontinental system could have been established, or the full intercommunicating value of the telephone developed. For it is one of the few natural monopolies. Foreign telephone experts say that the American telephone system has no equal in scope and efficiency, which is a gratifying endorsement, in view of the fact that the foundations of this American transcontinental system were not laid by men long skilled in an established art, or men who wielded the power inherent in great financial resources, but by men who strove against the combined forces of complete absence of telephonic experience, practise and knowledge, of the destructive power of the elements, and of human greed that would publicly rob vested right and good name.

Had these pioneers comprehended all that was to be endured, the losses, the bitter competition, the costly litigation, how many would have had the courage to imperil funds and business reputation in so hazardous an undertaking? For never before did an industry progress so rapidly as is recorded of the art of telephony, none ever had to face such costly, peculiar and ever-expanding demands, and none was ever so bitterly and so unjustly assailed.

These pioneers soon found that one set of telephone equipment would scarcely be installed by a local company before it would have to be displaced by improved apparatus, if the field was to be held. Or an unexpected marvelous growth in the number of subscribers would compel complete rebuilding of lines and the installation of more improved apparatus. Came storms of wind and sleet wrecking miles of pole line; flashed the lightning, burning out every coil in the plant; came the newly-invented electric lights rendering the service useless after night-fall until circuits were rearranged; came the trolley, making metallic circuits a technically and a judicially determined necessity; all in the brief span of eight years. 'Nothing is constant but change,' was a sentiment readily subscribed to by pioneer telephone men.

Yet notwithstanding discouragements, disasters and hardships, of a character unknown before, the dream of yesterday is the realization of to-day. For now there are nearly three million telephones connected to this one transcontinental system, receiving service over a total wire mileage exceeding five million miles, while the actual cash investment in *new construction alone* expended by the companies forming this great system during the *past five years only* aggregates two hundred millions of dollars.

How came the first commercial telephone exchange to be devised? is a question often asked. The idea of a central exchange telephone system was one of Graham Bell's earliest conceptions in connection with the possible utilization of the telephone. Thus it came in the natural development of so useful a public-service function as telephone

service. Prior to the exhibition of the telephone at the Centennial Graham Bell had frequently discussed with his partners, Mr. Hubbard and Mr. Sanders, the great value of a telephone exchange system covering an entire city, while in the lectures delivered in various cities during the winter of 1876–7 Graham Bell outlined many of the principal features that later were embodied in the early telephone exchanges.

However, it should be borne in mind that by reason of the existence in the larger cities of local telegraph central offices or exchanges, operating on lines somewhat similar to the early telephone exchange, the probable usefulness of a telephone system should have appealed to many whose experience with an intercommunicating system might have enabled them to forecast the development of the telephone exchange system. But the contrary appears true.

The central district-telegraph offices were in existence ten years before the invention of Bell's electric-speaking telephone. Through one 'central' the banks and the clearing-house were connected. Stock quotations and the premiums on gold were sent to brokers through another. Many lawyers maintained a 'central' in one city. In a second city the steel mills and factories were thus connected; while in a third city the newspapers maintained a telegraph exchange.

But none of these systems afforded communication other than by electrical apparatus mechanically operated, as, for instance, a dial telegraph, or a printing telegraph system, or an ordinary Morse key and sounder; apparatus and methods in no wise requiring the aid of the complex exchange mechanism known as the modern telephone switchboard. Nor were there any known means prior to 1876, of distant oral communication, aside from the speaking-tube. Yet, in many cities, the telegraph 'central' was the nucleus from which the respective local telephone exchange was evolved.

For after the newspapers and the magazines had made known the high esteem in which Sir William Thomson and other scientists held Graham Bell's telephone, a number of these 'central district' companies, operating electrical communicating circuits investigated the advisability of adding this new device as a side issue, or were invited to make a trial of the telephone in the belief that it would prove a good revenue producer. After investigating the merits of 'Bell's scientific toy' as many called it, some of these electric-service companies could perceive no profit in introducing this 'toy,' placed no commercial value on its serviceability in affording communication over distance, or comprehended its usefulness. And it may be safely stated that, in 1877, less than a score of men foresaw the marvelous future of the telephone or could grasp the meaning of its revolutionizing possibilities, or ever dreamed of such a phenomenal growth in so short a period as a quarter of a century. Even so experienced a man in the world's work as a former president of the Western Union Telegraph Company declared,

early in 1877, that 'the telephone could never be of any practical use in business affairs'; while men eminently successful in industrial and commercial circles ridiculed the inventor as 'the man who is trying to make the people believe you can talk through a wire,' and scoffed at his invention.

But Graham Bell's faith in the usefulness and the value of his invention and in its power to eliminate distance in many of the affairs of life never failed. He wasted no time lamenting over evil predictions of failure. To him life was rich in possibilities that come only in the dreams of the unselfish toiler for the welfare of others. And wisdom taught him that brooding never brought fruition. So, early in 1877, he and his partner, Gardiner Greene Hubbard, planned the telephone exchange system somewhat along the lines it developed later, including a trunking-system to connect the different exchanges in the same city, toll lines connecting adjoining cities, suggested the use of aerial cables and underground circuits, as a substitute for the many wires they clearly foresaw would be required in the near future, should the system prove a success, and discussed the advisability of adopting either a fixed rental or flat rate per month or of charging for each 'switch' or call, similar to modern measured-service methods.

And though several companies and many individuals failed in their efforts to successfully introduce the telephone in their respective localities, and gladly seized the opportunity to surrender the license previously obtained for a nominal payment, others were induced to continue in the telephone business, only through the earnest assurance of Graham Bell and his associates that they were planning a comprehensive system continental in its scope, and that, in perfecting these plans they were carefully considering every feature that would enable the public to rapidly derive the greatest possible benefit from Bell's invention.

That all these plans were thoughtfully considered and final decision made on a broad basis are clearly shown in the determination that the telephone should be leased and never sold, and that, while the installation of private lines yielded an immediate profit, the exchange system was the only true field for development. Thus it came about that gradually Graham Bell won staunch supporters to his way of thinking, who comprehended the true function of the telephone and perceived the commercial possibilities in the telephone exchange system. These broad-minded and enthusiastic adherents sought prospective customers among men who had long felt the need of a different method of communication from any then in vogue, and found these patrons on every hand.

As sufficient capital could not be secured in 1877–78 by Graham Bell's friends to enable one company to establish telephone exchange systems in a number of cities, the only recourse open was to afford every encouragement in the establishment of local plants by men of local

influence or hustling ability, who shared in the optimistic views of Graham Bell and Mr. Hubbard. While this latter plan would provide the quickest method of meeting the universal demand that Graham Bell was confident would develop, it might not prove the best plan for the public in the long run. For it meant a large number of different companies, governed by as many different policies, and operating under an endless number of systems. While under one company only one policy would prevail, system, equipment and methods would be standardized, and interconnecting lines built as rapidly as the necessary funds could be procured. Thus it was to determine which was the better plan, that Bell licenses were at first issued only for limited periods of five or ten years, with the understanding that at the expiration of the agreed term, the Bell company could take over the local exchange system at cost price, if it so desired.

Again it was quickly perceived that the true value of the telephone was intimately interwoven in the breadth and scope of the exchange system, and that its sphere of usefulness would be seriously curtailed unless combined with all the rights and privileges that a corporation secures from borough and township, county and state, rights that include the building of pole lines on highways and streets, the stringing of aerial circuits and the placing of cables underground. In other words, that the local value of telephone service was in direct ratio to the physical expansion of the exchange system, and in its capacity for meeting the ever-varying requirements of local users. Without the telephone the exchange system would have no value; without the exchange system the telephone would have comparatively little value.

By reason of the very nature of the invention, the right to use the telephone had to be safeguarded as few other patents have been, not only for the protection of the owner, but also that the licensee company might not suffer. A mowing-machine is a self-contained piece of mechanism that can be operated in any field, regardless of the number of or the absence of other mowers. The fewer mowing machines in a county the greater the probable profit of the owner of a machine intended for general use, and the greater its usefulness in a farming community. On the contrary, the fewer telephones in a county the more limited the value of the service to the public. While the greater the number of telephones and the more comprehensive the scope of exchange and toll system, the richer the benefits conferred upon the public, provided all telephones in the county are intercommunicating.

(To be continued.)

THE JEWS: A STUDY OF RACE AND ENVIRONMENT

By Dr. MAURICE FISHBERG
NEW YORK CITY

II. *Marriages.*

ONE of the most important causes of the low birth rate of the Jews is their low marriage rate. Only about fifty years ago an unmarried Jew was very rare in Europe, while an old maid was hardly to be met with in the Ghetto. They then followed closely the rabbinical ordinances: "It is the duty of every Israelite to marry as early in life as possible. Eighteen years is the age set by the rabbis; any one remaining unmarried after his twentieth year is said to be cursed by God Himself. Some rabbis urge that children should marry as soon as they reach the age of puberty, *i. e.*, the fourteenth year. A man, who, without any reason, refuses to marry after he has passed his twentieth year is frequently compelled to do so by court."[1] These Talmudical ordinances are not observed to-day by the bulk of European and American Jews, and their marriage rates are much below those of the christian populations among which they live.

Taking first statistics of the crude marriage rate, *i. e.*, the annual number of marriages per 1,000 population, we find in every country in Europe, where data are available, that the rates for the Jews are lower, as can be seen from the following table:

Country.	Period.	No. of Marriages per 1,000 Population.	
		Jews.	Christians.
Algeria	1903	9.12	8.17
Germany	1903	7.10	8.27
Prussia	1904	7.37	8.56
Bavaria	1903	7.80	7.70
European Russia	1897	7.37	9.48
Warsaw, Poland	1901	6.71	8.72
Roumania	1902	10.30	18.70
Hungary	1900	8.04	8.84
Bohemia	1900	7.24	8 26
United States	1886–90	7.40	——

It must be emphasized that even these figures do not give an adequate idea of the low marriage rates of the Jews, because the Jewish population contains a smaller number of children and larger proportion

[1] 'Jewish Encyclopedia,' Vol. VIII., p. 347.

of adults of marriageable ages. If statistics of the number of marriages per 1,000 Jews over fifteen years of age, and especially of unmarried adults, were obtainable, the rates for the Jews as compared with the christians in Europe would show that they are yet less apt to marry than the figures in the above table indicate. Thus it was found in Berlin during the census of 1900 that of all persons over twenty years of age the following percentage were married:

	Men.	Women.
Jewish	51.62 per cent.	52.51 per cent.
Christian	60.38 per cent.	53.83 per cent.

These low marriage rates, which are only a recent social phenomenon among the Jews, are not confined to western Europe. Even in Russia, where the bulk of the 5,000,000 Jews live to-day under strict adherence to their faith and traditions, early marriages are less frequent than among the christians. The poor muscular development of the eastern European Jews, which has in part been attributed to early marriages, will have to be explained by some other causes. The following figures, from the census of Russia of 1897, give the percentage of persons who married at certain ages, both among the Jews and among the general population of the so-called 'Pale of Settlement.'

Age.	Men.		Women.	
	Jews.	General Population.	Jews.	General Population.
20 and less	5.95	31.22	27.76	55.01
21 to 25	43.73	35.64	51.23	31.57
26 to 30	34.05	18.94	12.68	6.96
31 to 35	6.52	5.61	6.69	2.80
36 to 40	3.39	3.30	1.86	1.65
41 to 45	1.75	2.13	0.90	0.97
46 to 50	0.54	1.41	0.81	0.61
51 and over	3.07	1.65	1.07	0.42

It appears from this table that the Jews in Russia marry later in life than the christians in that country. Only 5.95 per cent. of all the Jews who married in 1897 were less than twenty years of age, while about five times as many christians married at this youthful age. Even among the Jewesses only 27.76 per cent. married before reaching twenty, as against double that number, 55.01 per cent., among christian women. Marriages between twenty and thirty, on the other hand, are more frequent among the Jews—77.78 per cent. as against only 54.58 per cent. among christians, and 63.91 per cent. among the Jewesses, and 38.53 per cent. among the christian women. Finally, marriages among persons at advanced ages, over thirty, are contracted in about the same proportions in both groups. In general, Russia has an extraordinarily large number of youthful marriages. Nowhere in

western Europe is there to be found more than four per cent. of bridegrooms under twenty years of age. This is of course due to the numerical predominance of agricultural workers with a communistic arrangement of the village community. The Jews in that country live mostly in cities, are either manufacturers, merchants, etc., or skilled mechanics. Marriage among such people must be postponed till business assures a secure income, or till the individual has attained skill in his trade or profession. The low birth rates of the Jews in Russia (compared with the non-Jewish population of that country) is here partly explained. The later an individual marries, the shorter is the period of the married state, and the number of children to be expected is smaller. In fact the census of 1897 showed that there were five per cent. more unmarried Jews than unmarried christians, as can be seen from the following figures:

Social State.	Jews.		Greek Orthodox.	
	Men.	Women.	Men.	Women.
Single	61.20	57.35	55.95	52.09
Married	36.76	36.07	40.28	39.42
Widowed	1.82	6.01	3.68	8.36
Divorced	0.16	0.49	0.03	0.04

In this connection it is interesting to note that the percentage of protogamous marriages is smaller among the Jews in Russia than among the general population. The marriages between bachelors and maids constituted 83.73 per cent. among the general population and only 80.72 per cent. among the Jews. Second marriages are more frequent among the Jews; and contrary to the almost general experience that widowers are more likely to marry maids than widows, the Jewish widowers in Russia more often marry widows. These peculiarities are explained by social customs.

We can find nothing in the above figures which would indicate any racial influence on the marriage rates of the Jews. In fact the evidence indicates that it is purely a social phenomenon. The Jews in eastern Europe marry earlier and have a smaller proportion of celibates than their coreligionists in western Europe, because the latter enjoy on the average a better social and economic prosperity. When compared with their non-Jewish neighbors in eastern Europe, the Jews have a lower marriage rate, because they have very few agricultural laborers, and have, on the other hand, a larger number of merchants, skilled workmen, etc., whose birth rate is lower among all peoples in Europe. In oriental countries, like Palestine, Turkey, Persia, Morocco, Algeria, Tunis, etc., where the Jews live under a primitive culture, and are entirely unaffected by any occidential influences, the Jews to-day marry very early. Husbands at fourteen and

wives at the same age are not uncommon. Their birth rate is also above that of the non-Jewish population in those countries, as was shown to be the case in Algeria.

In western Europe and America, where the Jews have been intensely influenced by the occidental social environment, their marriage rates are low. But even here conditions have not always been the same as we find them to-day. Statistics of marriage rates in the beginning of the nineteenth century show conclusively that then the Jews married earlier and had comparatively fewer celibates than the christian population of Germany. Their economic, social and cultural condition at that time was about the same as that of the Jews in eastern Europe to-day. Even as late as 1861 to 1870 Austrian statistics show that 34.3 per cent. of all the Jews who married were less than twenty-four years of age, as against only 17.6 per cent. of christians who married thus early; 23.5 per cent. of the Jewesses were married at this early age, and only 15.1 of christian women. Conditions have changed recently, as was shown above, going hand-in-hand with the change in the sum total of the social, economic and intellectual conditions in which the Jews find themselves at the beginning of the twentieth century.

Consanguineous Marriages

The extraordinarily large number of physical and mental defectives met with among the Jews in Europe has been in part attributed to the frequency of marriage of near kin among them. All available statistical evidence shows that consanguineous marriages are much more often contracted among Jews than among others. Jacobs, adopting Sir George H. Darwin's method, shows that in England 7.5 per cent. of all Jewish marriages are between cousins, while among Englishmen only two per cent. are of this class. Stieda found that in Lorraine the proportion of consanguineous marriages is 1.86 per thousand ordinary marriages among the protestants, 9.97 among catholics, and 23.02 among Jews. In Hungary marriage of near kin can only be contracted after a special permission has been obtained from the civil authorities. The data on the subject in that country are therefore reliable. During 1901 such permission was granted 270 times to Jews and 1,217 times to others. On a basis of the population, it thus appears that the Jews obtained proportionately about five and one half times as many permits as the christians. Among a thousand christian marriages 5.8 were between cousins, while among a thousand Jewish marriages there were 39.3. In Prussia the rates were in 1872–5, among the Jews 23.08 per thousand ordinary marriages; protestants, 14.68, and catholics, 9.98. From figures collected by Treitel in Berlin it appears that during 1900 the proportion of con-

sanguineous marriages was among the Jews 23.3, and among protestants only 6.1, per 1,000 marriages.

It is doubtful whether this inbreeding is the cause of most of the diabetes, insanity, idiocy, deaf-mutism, etc., encountered among the Jews. It is at present the consensus of opinion that consanguineous marriages contracted by healthy individuals are not at all detrimental to the offspring. But when contracted by defectives, the physical or mental defect is likely to appear in a more accentuated form in the progeny. It must in this connection be mentioned that consanguineous marriage among the European population rarely exceeds one per cent. of all marriages and is more frequent, as a rule, in the country than in the city. The Jews, as city dwellers, ought to have a still smaller proportion of marriages of near kin.

Mixed Marriages.

The prevailing opinion that Jews have always refrained from intermarriage with non-Jews is erroneous. Biblical tradition shows clearly that the ancient Hebrews intermarried freely with their Gentile neighbors. Some of the most important patriarchs (Abraham, Joseph, Moses) and kings (Solomon) had gentile wives. The prophets, Ezra and Nehemia, both arraigned the Hebrews for their carelessness in this respect and appealed to them to maintain the purity of the race of Israel. A study of their history shows that the Jews have always more or less intermarried with the races and peoples among whom they lived. This was particularly the case during the Hellenic period, and in Spain for some time before their expulsion. Only during medieval oppression and persecution, rigid confinement in the Ghettos and strict isolation have the Jews married exclusively among themselves.

In recent times the Jews again began to marry with christians in Europe and in America. In some countries this intermarriage has assumed such proportions as to threaten the integrity of Judaism. A survey of the census statistics published in various European countries reveals that wherever the Jews are subject to special adverse legislation, segregated in Ghettos and deprived of friendly social intercourse with their non-Jewish neighbors, very little or no intermarriage occurs. This is particularly the case in oriental countries, like Turkey, Persia, Morocco, etc., and also in semi-oriental countries, like Russia. There intermarriage is prohibited by law; unless the Jew first accepts christianity or mohammedanism, he may not marry a gentile. On the other hand, in western Europe, where he enjoys all the privileges of a free citizen, mixed marriages are of frequent occurrence. In fact, in some countries even more frequent than intermarriage between catholics and protestants.

Country.	Period.	To 100 Pure Jewish Marriages there are Mixed.		
		Husband. Christian.	Wife. Christian.	Total.
Copenhagen.....	1880–1903	—	—	65.38
Germany	1901–1904	8.01	9.26	17.27
Prussia	1875–1879	5.37	4.46	9.83
"	1880–1884	5.11	5.25	10.36
"	1885–1889	5.89	6.46	12.35
"	1890–1894	6.22	6.34	12.56
"	1895–1899	7.91	9.04	16.95
"	1900–1904	9.02	10.24	19.26
Berlin.....	1875–1879	16.43	19.64	36.07
"	1895–1899	13.07	21.05	34.12
"	1904	15.10	24.00	39.10
Bavaria.....	1876–1880	2.18	1.68	3.86
"	1881–1885	1.46	1.79	3.25
"	1886–1890	1.84	2.63	4.47
"	1891–1895	3.41	3.41	6.82
"	1896–1900	2.97	5.87	8.84
"	1903	4.18	3.05	7.23
Hesse	1866–1870	0.50	—	0.50
"	1871–1880	1.82	0.89	2.71
"	1881–1890	2.82	1.59	4.41
"	1891–1900	3.06	1.90	4.96
"	1901–1904	3.16	4.17	7.33
Amsterdam	1899–1901	—	—	9.45
"	1902–1904	—	—	15.08
Hungary.....	1895–1903	2.98	2.97	5.95
Budapest.....	1896–1900	7.22	6.11	13.33
"	1901–1902	7.86	6.98	14.84
"	1903–1904	8.22	8.84	17.06

The German censuses are particularly reliable because they have been collected for many years with great care, with a view to eliciting the degree of assimilation of the Jewish population. During the four years 1901 to 1904 there were contracted in Germany 15,635 marriages between Jews and Jewesses, and 2,700 between Jews and christians. The mixed marriages constituted consequently 17.27 per cent. of the pure Jewish marriages, or one sixth of all the Jews who married married christians. It was also found that 8.01 per cent. of all the Jewesses and 9.26 per cent. of all Jews in that country married christians. In other words, every twelfth Jewish bride and every eleventh Jewish bridegroom married a christian.

The largest number of mixed marriages in Germany are contracted in the province of Prussia. There statistics for twenty-eight consecutive years are available (1876–1904), during which 71,160 pure Jewish marriages were contracted; 4,740 between christians and Jewesses and 5,062 between Jews and christian women. It was also established that the number of mixed marriages is constantly on the increase, as can be seen from the following figures:

	Average Annual Number.
1875–1879	239
1895–1899	433
1900–1904	495

In Berlin the proportion of mixed marriages is about twice as large as in Prussia generally. During 1875–9 the average annual number was 101; during 1895–9 it was double, 201; during 1898–1902, 212, and in 1904 it rose to 246. During the last mentioned year 24 per cent. of all the Jews who married married christian women, and 15.1 of the Jewesses married christians. In other words, every fourth Jew and every seventh Jewess in Berlin who married during 1904 married a christian.

In the other provinces of Germany mixed marriages are also more or less frequent. The record for Hamburg is, during 1896 to 1900, that 5 per cent. of Jewesses and 8.3 per cent. of Jews marry outside of their faith. In Bavaria during the twenty-five years, 1876–1900, it was found that to every 100 pure Jewish marriages contracted, 5.35 were contracted between Jews and christians. That these marriages are on the increase is shown in the table on page 446. The percentage was 3.87 in 1876–80 and 9.0 in 1904. Similarly in Hesse, where in 1866–70 only one in 200 Jews who married, married a christian, mixed marriages have since been on the increase, so that during 1901–4 the mixed marriages amounted to 7.33 per cent. of the pure Jewish marriages. Even in Amsterdam, where the most orthodox Jews are living, and up to about fifty years ago, hardly any Jew married out of his faith, there are to-day a very large number of such unions. During 1899–1901 the mixed marriages constituted 9.45 per cent. and during 1902–3 the proportion increased to 15.08 per cent.

The largest percentage of mixed marriages are contracted in Copenhagen, Denmark, where statistics, recently compiled by Julius Salomon, show that during the twenty-four years, 1880 to 1903, there were contracted 358 pure Jewish marriages and 234 mixed marriages, or 65.36 per cent. of the pure Jewish marriages. How far these marriages are, in that city, in vogue among the Jews is seen by the fact that from 1880 to 1890 the percentage was only 55.17; it rose to 71.03 per cent. during 1891 to 1900, and from 1901 to 1903 it was already 89.74 per cent. In former years we are told that the Jewish rabbis refused to admit children born from mixed marriages to Judaism, but of late years the Jewish community in that city authorized several Jewish physicians to circumcize such new-born boys. This is not confined to Copenhagen alone, but is characteristic of Scandinavia in general. In Denmark there were contracted, during 1873–91, 308 Jewish marriages, of which 187 were pure and 121 mixed, *i. e.*, 64.71 per cent. Since then the increase has been enormous. In the neighboring country, Sweden, it is stated that the number of mixed marriages is much in excess of the number of pure Jewish marriages.

In France and Italy also Jews frequently marry christians. This is particularly the case with the French aristocracy, who often marry Jewish heiresses.[2] In Italy the Jews to-day are thoroughly assimilated, and many observers state that mixed marriages are almost as frequent as pure marriages.

In eastern Europe they are less frequent. In Austria, during 1901, there were contracted 7,576 pure Jewish marriages and 147 mixed marriages. But here we can see how far isolation acts as a preventive of intermarriage. Of the 147 mixed marriages contracted during 1901, 98 were in the city of Vienna and 25 in Bohemia. Although three quarters of all the Austrian Jews live in Galicia, still not a single case of intermarriage was recorded there during that year. It must also be mentioned that in Austria intermarriage between Jews and christians is not permitted by the law, and in many cases of mixed marriages, one of the parties adopts the religion of his or her partner, and the marriage is thus recorded as pure christian or pure Jewish; or one or both declare themselves as dissenters (Konfessionslos) and appear on the registration lists as the marriage of a Jew with a dissenter, or of dissenters. As a result of this condition the available figures do not by any means represent the true condition of affairs.

Up to 1895 intermarriage was entirely prohibited by law in Hungary, unless one of the parties was converted to the religion of his or her partner. Since this law was abolished in 1895, mixed marriages are taking place in large numbers. During the nine years, 1895 to 1903, 3,590 Jews married with christians, and 60,275 with Jews, *i. e.*, 5.95 per cent. of all pure Jewish marriages were mixed. Most of these marriages are contracted in the city of Budapest, where the proportion reached 17.06 per cent. during 1903 and 1904. The steady increase of mixed marriages in that city is well seen from the following figures:

	1896–1900	1901–1902	1903	1904
Jews to christian women.....	6.71	6.98	8.41	7.86
Christians to Jewesses.....	7.22	7.86	7.85	7.36

It thus appears that every thirteenth Jew who married in Budapest, during 1904, married a christian.

For English-speaking countries there are no available statistics on the subject of intermarriage, because no religious censuses are taken. In England they occur often among the native Jews, and although among the immigrant Jewish population in London they are less frequent, still they are not as rare as is generally believed. In New South

[2] It has been alleged that most of the mixed marriages are contracted between christian noblemen and rich Jewish heiresses. This is disproved by the figures in the table of mixed marriages. The proportion of Jews who marry christian women is larger than that of christians who marry Jewesses.

Wales it was found, while taking the census of 1901, that of all married Jews, 781 were married to Jewesses and 686, *i. e.*, 87 per cent., were married to christians.[3]

In the history of the Jews in the United States there are many instances of intermarriage between Jews and christians, even in Colonial times. According to Professor Hollander, the well-known 'Ye Jew doctor,' Jacob Lumbrozo in Maryland married a christian woman about 1660.[4] Dembitz shows that "there is no frequenter of the synagogue who either lived in Kentucky or whose ancestors lived there before 1836," and he gives as a cause that the early Jewish settlers disappeared through intermarriage with christians "and the descendants of the early Jewish settlers are known only by their Jewish family names and their oriental (?) features."[5] One has to read detailed accounts of several Jewish families in New York, Pennsylvania, Connecticut, Massachusetts, etc., to be convinced as to the extent of mixed marriages in pre-revolutionary times. The Franks family is particularly interesting: One daughter, Rebecca, married Sir Henry Johnson; another, Mary or Polly, married Andrew Hamilton.[6] About New York, M. J. Kohler says in his work 'Jewish Life in New York before 1800' that "several cases are at hand of intermarriage between Jews and Jewesses to christians and occasional conversions to the prevailing religion."[7] In the 'Biographical Sketches of the Graduates of Yale College,' Vol. II., 1763, two Jews are mentioned, one 'of Jewish extraction' who became a prominent citizen and one of the founders of the episcopal church in Norwalk; the other married a woman of French Huguenot descent. In Ohio also all traces of the early Jewish settlers have been lost. One is mentioned who was married 'out of his faith,' but when he died, in 1821, he asked to be buried with Jewish rites.[8] Speaking of Judah P. Benjamin, of New Orleans, whose wife was a devout catholic and whose daughter married Captain Henri de Bousignac, of the 117th regiment of the French line, Kohler says: "Such intermarriage was, in 1833, not uncommon." A Jewish traveler in New Orleans in 1842 speaks of the synagogue, which merely accommodated fifty persons, and a former "rabbi, a Dutchman, had married a catholic wife, who with difficulty was restrained from sending a crucifix to his grave at his burial."[9]

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[3] Census of N. S. W., 1901, Bull. No. 14.

[4] Public. Jewish Histor. Soc., I., p. 29.

[5] *Ibid.*, pp. 99–101.

[6] Westcott, 'Historic Mansions,' quoted from Publ. Jew. Histor. Soc., I., pp. 57–58.

[7] *Ibid.*, II., p. 91.

[8] *Ibid.*, VII., p. 43.

[9] *Ibid.*, XII., pp. 68–69.

In more recent times intermarriage of Jews with christians in the United States has continued unabated. In fact, very few of the original Jewish settlers, who were of Spanish and Portuguese origin, have left descendants. Most have been absorbed by intermarriage.[9] For lack of denominational statistics, it is impossible to state accurately the extent of intermarriage between Jews and christians in the United States at the present time. It is known to be very common in the western and southern states, and less so in the eastern states. In a recent paper by Professor M. Schlesinger, of the Hebrew Union College, he quotes Rabbi George Zepin, director of circuit preaching, to the effect that in the northern part of the United States five per cent. is the maximum proportion of mixed marriages, while in the south the proportion ranges from 20 to 50 per cent., 33 per cent. being most nearly correct.[10]

There are no available statistics as to the frequency of mixed marriages in New York City to-day. From the census of the Federation of Churches taken in 1902 of the Twenty-second Assembly District (37th to 55th streets, east side), there were found several cases, about one per cent., of intermarriage. But this does not by any means represent the real conditions. Among many of the older Jewish families in this city we find many cases of intermarriage, and even on the east side, among the immigrant Jews, they are no more rare. In the western states it is very common, and the rabbis have of late actually sounded an alarm as to the danger of mixed marriages.

We can conclude with the words of Arthur Ruppin, who thoroughly studied the problem in Europe: "The best preventive of intermarriage is the Ghetto. In Galicia, Russia, the east end of London and the east side of New York, they are rare. But in countries where the Jews participate in the social and economic life of the general population as equals mixed marriages do occur and are steadily increasing in frequency, as is the case in Prussia, Denmark, Hungary, Italy, the western states, etc." The largest proportion of mixed marriages in Europe is contracted in large cities, as we see from the figures given above is the case in Berlin, Vienna, Budapest, Copenhagen, etc. This is because in large cities the population (Jewish as well as christian) has a higher intellectual standard, and these are better opportunities for people of different faith to come into intimate contact with each other.

[10] See Public. Jewish Histor. Soc., Vol. VI., pp. 92–93.

(To be continued.)

JOHN STUART MILL

By PERCY F. BICKNELL

"I FIND it hard to say why I dislike John Stuart Mill," writes Lowell to Leslie Stephen, "but I have an instinct that he has done lots of harm."

"For the sake of the House of Commons at large," says Gladstone in a letter to Mr. W. L. Courtney, Mill's biographer, "I rejoiced in his advent, and deplored his disappearance. He did us all good. In whatever party, whatever form of opinion, I sorrowfully confess that such men are rare." "A wiser, more virtuous man I have never known, and never hope to know," was Mr. John Morley's pronouncement in a speech delivered soon after Mill's death.

To continue a little further these contrasting opinions concerning a philosopher and reformer, the centennial recurrence of whose birthday directs our thoughts upon him at this time, we find Professor Jevons somewhat petulantly exclaiming: "For my part I will no longer consent to live silently under the incubus of bad logic and bad philosophy which Mill's works have laid upon us. . . . In one way or another Mill's intellect was wrecked. The cause of injury may have been the ruthless training which his father imposed upon him in tender years; it may have been his own life-long attempt to reconcile a false empirical philosophy with conflicting truth. But however it arose, Mill's mind was essentially illogical."

"Mill's intellect was essentially of the logical order," declares his biographer and expounder, Sir Leslie Stephen. The late E. L. Godkin called him "the most accomplished of modern dialecticians." Herbert Spencer, referring to Mill's influence on current English philosophical speculation, was of opinion that "by his 'System of Logic' Mr. Mill probably did more than any other writer to awaken it." Henry Sidgwick praised "the unequalled mastery of method which his logical speculations developed."

Now that Mill has been dead a third of a century, it may be worth while to take the occasion of this hundredth anniversary of his birth to review briefly the estimation in which he was held by his contemporaries, and to consider how much and what part of his fame of thirty-three years ago is now alive and likely to survive. His lasting influence, whether for good or for ill, is of course not accurately determinable; for, as Professor Bain has well said, "no calculus can integrate the

innumerable little pulses of knowledge and of thought that he has made to vibrate in the minds of his generation"—and, one may add, in the minds of the succeeding generation.

That keen, alert intelligence, trained, as an athlete is trained, to the very maximum of efficiency, if not indeed overtrained, can not but command the admiration of Mill's readers to-day, as it did that of his contemporaries. One can hardly read his remarkable 'Autobiography' without acknowledging in Mill a kind and a degree of intellect so far out of the ordinary as to approach the præter-human—we will not say the superhuman. "Logic-chopping engine," Carlyle may dub his old friend, as he emphatically does in describing the 'Autobiography' to his brother John. "I have never read a more uninteresting book," he declares, "nor should I say a sillier, by a man of sense, integrity, and seriousness of mind. . . . It is wholly the life of a logic-chopping engine, little more of human in it than if it had been done by a thing of mechanized iron. Autobiography of a steam-engine." "A fascinating book it is from beginning to end," said Edward Everett Hale, in reviewing the work at the time of its appearance.

Probably the one book of Mill's that has done more than any other to awaken interest and arouse enthusiasm is his treatise on 'Liberty.' Its merits are well indicated in an adverse criticism that occurs in one of Caroline Fox's letters. "I am reading," she says, "that terrible book of John Mill's on Liberty, so clear and calm and cold; he lays it on one as a tremendous duty to get oneself well contradicted, and admit always a devil's advocate into the presence of your dearest, most sacred truths, as they are apt to grow windy and worthless without such tests, if, indeed, they can stand the shock of argument at all. He looks you through like a basilisk, relentless as fate. . . . No, my dear, I don't agree with Mill, though I, too, should be very glad to have some of my 'ugly opinions' corrected, however painful the process; but Mill makes me shiver, his blade is so keen and so unhesitating." It is exactly this willingness, and more than willingness, to hear the other side, that attracts the young reader, and in fact all ingenuous minds, to Mill. His writings bear the unmistakable stamp of sincerity. "I had always," he tells us, "a humble opinion of my own powers as an original thinker, . . . but thought myself much superior to most of my contemporaries in willingness and ability to learn from everybody; as I found hardly any one who made such a point of examining what was said in defense of all opinions, however new or however old, in the conviction that even if they were errors there might be a substratum of truth underneath them, and that in any case the discussion of what it was that made them plausible, would be a benefit to truth."

In harmony with this spirit of modesty and candor is that peculiar quality in his writings which is at the same time one of their chief

merits and a considerable bar to the recognition of their originality. In whatever field of learning he worked, he always sought to knit his thoughts into the body of pre-existing knowledge, and to make his current of speculation flow easily and naturally from sources already familiar to his readers, however widely that current might afterward diverge from the well-worn channels. Thus he was actually at more pains to conceal his originality than most writers are to bring theirs into prominence. In political economy he wrote as an expounder and popularizer of Ricardo, in morals as a disciple and interpreter of Bentham, in the philosophy of mind as a commentator on the works of his father and of Sir William Hamilton, and even in his 'Logic' he is so scrupulously careful to acknowledge indebtedness to earlier thinkers that an undiscerning reader might easily undervalue Mill's contributions in his own name. Recognizing the breadth and fullness of his mind, one is in danger of doing less than justice to its originality.

Mill has been compared with Locke, his influence on the nineteenth century being likened to the earlier philosopher's on the seventeenth. As parts of Locke's teachings have long since passed into the body of common thought and conviction, and have thus lost for us their originality and interest, so there are many of Mill's doctrines that have in this best sense become obsolete, because by general adoption they have ceased to be matter of argument. In addition to the practical reforms he inaugurated or promoted, we may ask at this time, what is his significance and value to us as a philosopher? By example as well as precept he has elevated and purified the utilitarian scheme of ethics. The greatest-happiness principle was with him a religious principle. We may hold that he was fundamentally wrong in his theory of morals, but we can not refuse to applaud his practise. In the abstruser regions of thought, his neat and clear exposition of the experience-philosophy is suggestive rather of the French than of the German school. One can hardly read three pages of German metaphysics without a depressing sense of the futility of human reasoning, whereas a French philosophical treatise fills us with a surprised delight at the efficiency of our own powers. The German is too fond of directing our gaze into fathomless abysses and of leading our feet into bottomless quagmires; the Frenchman conducts us easily and pleasantly over a macadamized road, where all steep ascents are carefully graded, precipitous declivities guarded against by walls and fences, and ugly or disquieting outlooks screened by flowering hedges. As Martineau long ago so admirably expressed it, Mill's distinguishing characteristics as a philosopher are "sharp apprehension of whatever can be rounded off as a finished whole in thought, analytic adroitness in resolving a web of tangled elements and measuring their value in the construction, reasoning equal to any computation by linear coordinates, though not readily

flowing into the organic freedom of a living dialectic, remarkable skill in laying out his subject symmetrically before the eye and presenting its successive parts in clear and happy lights. No one has more successfully caught the fortunate gift of the French men-of-letters—the art of making readers think better of their own understanding and less awfully of the topics discussed." The same keen critic points out a glaring self-contradiction in Mill's theorizing. Mill resolves all knowledge into self-knowledge, since we have no cognitive access to either qualities or bodies external to ourselves. On the other hand, however, we know nothing but the phenomena of ourselves, we are but phenomena of the world, and the sensations from which all within us begins are merely the results of outward experience. Thus the pretended *a priori* ideas turn out to be *a posteriori* residues; the volitions that claim to be spontaneities are necessary effects of antecedent causes earlier than we. "And thus," the critic well concludes, "we are landed in this singular result: our only sphere of cognitive reality is subjective: and that is generated from an objective world which we have no reason to believe exists. In our author's theory of cognition, the non-ego disappears in the ego; in his theory of being, the ego lapses back into the non-ego. Idealist in the former, he is materialist in the latter."

We find, then, that in matters of abstract speculation Mill produced little that will live. But where he could bring his thought to the service of humanity, his achievement is noteworthy; and for this we honor him. Even in his contributions to inductive logic, of which he is often called the founder, he was working for the enlightenment of human error in the practical concerns of life; how much more so in his political and economic writings, their greater concreteness makes evident. He was far from being a philosopher for the mere love of 'divine philosophy.' There was no art-for-art's-sake enthusiasm in him. For a luxury of that sort he had too little tendency to passive enjoyment, and too much of the militant, apostolic fervor of the reformer. The will-o'-the-wisp pursuit of ultimate truth for its own sake might well have seemed to him, as did philosophical speculation in general to an eminent contemporary of his, very much like the motions of a squirrel in its cage. Mill's studies demanded a humanitarian motive, and that motive became with him a religion. He himself, in his review of Comte, declares: "Candid persons of all creeds may be willing to admit, that if a person has an ideal object, his attachment and sense of duty towards which are able to control and discipline all his other sentiments and propensities, and prescribe to him a rule of life, that person has a religion." Mill's two chief characteristics, the love of thinking out difficult problems, and the love of mankind, were made to serve each other; and the gratification of these two passions may be regarded as the expression of his natural piety.

His adroitness in applying abstract principles to concrete realities, and thus making attractive to the many those studies of his that might otherwise have repelled even the few, is too well known to require illustration. As Herbert Spencer somewhere makes illuminative use of the shape of a present-day 'milk-jug' to illustrate the irrationality of fashion and convention, so Mill can strengthen an argument by happily introducing the diminishing vogue of fainting-fits among young ladies. And as in small things, so in large. The high degree of common sense inwrought in the philosophy of Mill and Spencer contributes no little to the readableness, the intelligibility and the popularity of their writings. That Mill was nothing but 'a book in breeches,' as he was so often called, can not rightly be made to appear, even in the most learned of his published works. Unless precision and clearness of thought, accuracy of expression, aptness of illustration, breadth of reading and of observation, and constant openness to conviction, constitute the pedant, he was no pedant. One may even wish that there were a little more of the bookish element in him; for, remembering the extent of his reading in both ancient and modern literature, we feel some disappointment at finding in his works so little of that common stock of graceful allusion and happy quotation that might have been expected to adorn and to light up his somewhat sombre pages. Nevertheless he can not properly be called 'a thing of mechanized iron.' If he was the 'steam-engine' that Carlyle pronounced him to be, he was at least an engine of that excellent sort that burns its own smoke, which is more than can be said of Carlyle.

Mr. Frederic Harrison, who knew Mill personally, is emphatic in asserting that his heart was "even richer than his brain." Mr. Morley places Mill's distinction in the "union of stern science with infinite aspiration, of rigorous sense of what is real and practical with bright and luminous hope." All readers will recall the purple patches in 'The Subjection of Women.' In spite of his proof armor of dry logic, the author is more than once carried away by what has been styled 'the logic of feeling.' Mr. Harrison calls him "excessively sensitive and indeed impressionable." As Condorcet said of Turgot, he resembled a volcano clothed in ice. Proofs of this warmth of feeling could be adduced in great number, but a very few must here suffice. For a whole year he took upon himself the duties of his friend and subordinate in the India House, W. T. Thornton, to enable the latter to recruit his health without relinquishing his post. Mill's offer to guarantee the expense of certain early publications of Spencer's and Bain's, and also his generous kindness to Comte, when the French philosopher had fallen on evil days, and at a time when Mill himself was suffering from heavy pecuniary losses, are matters of common knowledge. A considerateness for others, and a depreciation of self, that went even to extremes, may

be seen in Mill's conduct on retiring from the India House in 1858 after thirty-five years of service. His friends in the Examiner's Office, including every member of the force, desired to present him with a suitable token of regard. In half an hour after the matter was proposed, subscriptions were eagerly volunteered to the amount of fifty or sixty pounds, while outside contributions were jealously refused, as those in immediate service under the retiring examiner insisted on sharing with no outsiders the pleasure and honor of making this testimonial. But before the gift, an elaborate silver inkstand, could be got ready, the one for whom it was designed caught the scent and was greatly displeased. Approaching the originator of the plan, W. T. Thornton (as will have been surmised), he almost upbraided him, and was really angry, so far as it was in him to cherish anger. He said he hated all such demonstrations; was sure they were never wholly genuine; there were always some who took part in them only because they disliked to refuse; and, in short, he positively would have none of it. With him it was a question of principle, and where a principle was involved he could not give way, despite the obvious awkwardness in store for Thornton and his associates. The matter had gone too far to be dropped altogether, and finally the assistance of Mill's stepdaughter, Helen Taylor, was invoked; the inkstand was smuggled into the house without Mill's knowledge, and, thanks to Miss Taylor, instead of being promptly returned, it was in the end promoted to a place of honor in the drawing-room. Mill's excessive devotion to his wife, a devotion that manifests itself in some of his writings as idolatrous worship, proves the warmth of his heart, however clearly it may betray a lamentable clouding of the judgment by a passion to which he, of all men, had seemed least likely to fall a victim.

From the many who knew Mill in his lifetime, abundant testimony could be quoted to prove the charm and purity of his nature, as well as the intellectual and moral stimulus of his personality. "Intimacy with Mr. Mill convinced me," says Henry Fawcett, "that, if he had happened to live at either of the universities, his personal influence would have been no less striking than his intellectual influence. Nothing, perhaps, was so remarkable in his character as his tenderness to the feelings of others, and the deference with which he listened to those in every respect inferior to himself. There never was a man who was more entirely free from that intellectual conceit which breeds disdain. Nothing is so discouraging and heart-breaking to young people as the sneer of an intellectual cynic. A sarcasm about an act of youthful mental enthusiasm not only often casts a fatal chill over the character, but is resented as an injury never to be forgiven. The most humble youth would have found in Mr. Mill the warmest and most kindly sympathy." An anecdote from the same source illustrates another equally admirable

trait—an intellectual liberality unusual among scholars devoted to some chosen branch of study. "Some years ago," Fawcett narrates, "I happened to be conversing at Cambridge with three men who were respectively of great eminence in mathematics, classics and physiology. We were discussing the inaugural address which Mr. Mill had just delivered as rector of the St. Andrews University. The mathematician said that he had never seen the advantages to be derived from the study of mathematics so justly and so forcibly described; the same remark was made by the classic about classics, and by the physiologist about natural science. No more fitting homage can probably be offered to the memory of one to whom so many of us are bound by the strongest ties of gratitude and affection, than if, profiting by his example, we endeavor to remember that above all things he was just to his opponents, that he appreciated opinions from which he differed, and that one of his highest claims to our admiration was his general sympathy with all branches of knowledge."

What we of to-day owe to Mill, it seems safe to assert in closing, is not so much the advancement of learning in any particular direction —for the world has already caught up with and assimilated a great part of the new truth uttered by him—as the stimulus of a rarely pure and lofty and strenuous nature, devoted to high ideals for the amelioration of mankind, and unflinchingly courageous in advocating them by example as well as by precept. Industrious and versatile to a degree that astonishes one on surveying the products of his literary and his business activity, he at the same time achieved, in whatever he undertook, a uniformly high quality of workmanship that would be noteworthy even in the most rigorous specialist. To the strenuous youth of this strenuous age (if one may be pardoned for using again a much over-worked adjective) Mill may well serve as a model of nobly directed activity, generous self-sacrifice, and memorable achievement. But in emulating his example, let us first ponder well these words of his from a letter to Caroline Fox: "No one should attempt anything intended to benefit his age, without at first making a stern resolution to take up his cross and to bear it. If he does not begin by counting the cost, all his schemes must end in disappointment; either he will sink under it, as Chatterton, or yield to the counter-current, like Erasmus, or pass his life in disappointment and vexation, as Luther did."

CHANGES OF CLIMATE

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Popular Belief in Climatic Change.—Belief in a change in the climate of one's place of residence, within a few generations, and even within the memory of living men, is widespread. It is confined to no special region or people. It finds support among the most intelligent as well as among the uneducated. Here it may be the view that the climate is growing milder; there, that the winters are becoming more severe; here, that there is increasing aridity; there, that the rainfall is greater. Whenever a season attracts attention because of weather conditions which seem in any way unusual, this belief is strengthened. This popular impression has often found support in the facts of distribution, or the dates of flowering or ripening of certain cereals or fruits. It is asserted that because grapes, or corn, or olives, for example, are now no longer grown in parts of Europe where their cultivation was once an important occupation, we must conclude that the climate has changed from a favorable to an unfavorable one.

Evidences of Climatic Changes within Historical Times.—Evidence is constantly being brought forward of apparent climatic variations of greater or less amount which are now going on. Such reports, largely those of travelers or explorers in little-known regions, are usually based on fluctuations in the extent of inland lakes; on the discovery of abandoned dwelling sites, the ruins of aqueducts and irrigating canals, and the like. Thus we have accounts of a gradual desiccation which seems to have been going on over a large region in Central Asia, during historical times. In eastern Turkestan the lakes have been reported as drying up, Lake Balkash falling one meter in about fifteen years, and Lake Alakul gradually becoming a salt deposit. In his work on Turkestan, Muschketoff gives numerous examples of progressive desiccation, and Rossikoff speaks of the drying up of the lakes on the northern side of the Caucasus. The same thing is reported of lakes in the Pamir. Prince Kropotkin believes that the desiccation of Central Asia in the past drove the inhabitants out on to the lowlands, producing a migration of the lowland peoples and thus bringing on the invasions of Europe during the first centuries of our era. In his recent work on the basin of eastern Persia and Sistan, Huntington believes that, so far as it can be made out, the history of Sistan also

indicates a gradual desiccation from early historical times down to the present day. His study of climatic changes in that region is one of the most thorough ever made, for the evidence of archeology, of tradition, of history and of physiography has been carefully matched and found to accord in a very striking manner. Huntington has found evidence of the abandonment of successive village sites as the inhabitants moved further upstream in search of more water. Patches of dead jungle show that vegetation once flourished where aridity now renders plant growth impossible.

In northern Africa certain ancient historical records have been taken by different writers to indicate a general decrease of rainfall during the last 3,000 or more years, the remains of cities and the ruins of irrigating works pointing to a larger population and a greater water supply formerly than at present. The presence of certain animals, now no longer found there, is implied by ancient records, and from this fact also a change of climate is inferred. In his crossing of the Sahara between Algeria and the Niger, Gautier found evidence of a former large population. A gradual desiccation of the region is, therefore, believed to have taken place, but to-day the equatorial rain belt seems to be again advancing farther north, giving an increased rainfall. Gautier divides the history here into three periods, (1) dense population, (2) aridity, and (3) the present change to a steppe character.

Farther south, several lakes have been reported as decreasing in size, *e. g.*, Chad, Ngami and Victoria; and wells and springs as running dry. In the Lake Chad district, Chevalier reports the discovery of vegetable and animal remains which indicate an invasion of the Sudan by a Saharan climate. Neolithic relics indicate the former presence there of prosperous communities. Again, to note another instance, it is often held that a steady decrease in rainfall has taken place over Greece, Syria and other eastern Mediterranean lands, resulting in a gradual and inevitable deterioration and decay of their people. These examples might be multiplied, for reports of climatic changes of one kind or another are numerous from many parts of the globe.

What Meteorological Records show.—As concerns the popular impression regarding change of climate, it is clear at the start that no definite answer can be given on the basis of tradition, or of general impression, or even of the memory of the 'oldest inhabitant.' Human memories are very unreliable things, and there are many reasons for their being particularly untrustworthy in matters of this kind. The only answer of real value must be based on what the instrumental records of temperature, and of rain and snowfall show. Accurate instruments, properly exposed, and carefully read, do not lie; do not

forget; are not prejudiced. When such instrumental records, scattered though they are, and difficult as it is to draw general conclusions from them, are carefully examined, from the time when they were first kept, which in a few cases goes back about one hundred and fifty years, there is found no evidence of any progressive change in temperature, or in the amount of rain and snow. Apparent signs of a permanent increase or decrease in one or another element have been fairly easy to explain as due to the method of exposing the thermometer or of setting up the rain gauge. Little care was formerly taken in the construction and location of meteorological instruments. They were usually in cities, and as these cities grew, the temperature of the air was somewhat affected. The rain-gauges were poorly exposed on roofs or in court-yards. The building of a fence or a wall near the thermometer, or the growth of a tree over a rain-gauge, was enough, in many cases, to explain any observed change in the mean temperature or rainfall. Even when the most accurate instrumental records are available, care must be taken to interpret them correctly. Thus, if a rainfall or snowfall record of several years at some station indicates an apparent increase or decrease in the amount of precipitation, it does not necessarily follow that this means a permanent, progressive change in climate, which is to continue indefinitely. It may simply mean that there have been a few years of somewhat more precipitation, and that a period of somewhat less precipitation is to follow.

For the United States, Schott, some twenty years ago, made a careful study of all the older records of temperature and rainfall, including snow, from Maine to California, and found nothing which led to the view of a progressive change in any one direction. There was evidence of slight variations of temperature, occurring with the same characteristics and with considerable uniformity over large areas. These variations have the characteristics of irregular waves, representing slightly warmer and slightly cooler periods, but during the fluctuations the temperature differed by only a degree or two on one side or the other of the mean. Obviously, this is too slight a range to be of any general or practical interest, and in any case, these oscillations give no evidence of a continuous change toward a warmer or a cooler climate. Schott found that these waves of higher and lower temperature followed one another at intervals of about twenty-two years on the Atlantic coast. In the interior the intervals were about seven years. The records of the closing of rivers, the Hudson, for example, to navigation, show no permanent change in the dates for the last hundred years or so.

It has been well pointed out that if a list were carefully compiled of heavy snowstorms, of droughts, of floods, of severe cold, of mild winters, of heavy rains, and of other similar meteorologic phenomena

for one of the early-settled sections of the United States, beginning with the date of the first white settlements and extending down to the present day, we should have the following situation: Dividing this list into halves, each division containing an equal number of years, it would be found, speaking in general terms, that for every mild winter in the first half there would be a mild winter in the second; for every long-continued drought in the first division there would be a similar drought in the second; for every 'old-fashioned' winter in the first group there would be an 'old-fashioned' winter in the second. And so on through the list. In other words, weather and climate have not changed from the time of the landing of the earliest pilgrims on the inhospitable shores of New England down to the present day.

Why the Popular Belief in Climatic Changes is Untrustworthy.— Why is the popular belief in a change of climate so widespread and so firmly fixed, when instrumental records all go to show that this belief is erroneous? It is not easy to answer this question satisfactorily, but several possible explanations may be given. The trouble arises chiefly from the fact that we place absolute trust in our memories, and attempt to judge such subtle things as climatic changes on the basis of these memories, which are at best short, defective, and in the highest degree untrustworthy. We are likely to exaggerate past events; to remember a few exceptional seasons which, for one reason or another, made a deep impression on us, and we thus very much overrate some special event. To make use of an illustration given by another, individual severe winters which, as they occur, may be some years apart, seem, when looked back upon from a distance of several years later, to have been close together. It is much as in the case of the telegraph poles along a railroad track. When we are near the individual poles they seem fairly far apart, but when we look down the track, the poles seem to stand close together. The difference in the impressions made upon youthful and adult minds may account for part of this misconception regarding changes of climate. To a youthful mind a heavy snowstorm is a memorable thing. It makes a deep impression, which lasts long and which in later years, when snowstorms are just as heavy, seems to dwarf the recent storms in comparison with the older. The same is true regarding heavy rains, or floods, or droughts.

Changes of residence may account for some of the prevailing ideas about climate. One who was brought up as a child in the country, where snow drifts deeply and where roads are not quickly broken out, and who later removes to a city, where the temperatures are slightly higher, where the houses are warmer, and where the snow is quickly removed from the streets, naturally thinks that the winters are milder and less snowy than when he was a boy. Similarly, a change of residence from a hill to a valley, or *vice versa,* or from the coast to the

interior, may easily give the impression of a changing climate. Even in cases where individuals have kept a record of thermometer readings during a long series of years, and are sure that the temperatures are not as low or as high as they used to be, or who are convinced that the rainfall is lighter or heavier than it was some years before, the chances are that the location of the thermometer or the exposure of the rain gauge has been changed sufficiently to account for any observed difference in the readings.

Value of Evidence concerning Changes of Climate.—The body of facts which has been adduced as evidence of progressive changes of climate within historical times is not yet sufficiently large and complete to warrant any general correlation and study of these facts as a whole, especially from the point of view of possible causation. But there are certain considerations which should be borne in mind in dealing with this evidence, certain corrections, so to speak, which should be made for possible controls other than climatic, before conclusions are reached in favor of climatic changes. In the first place, it has been noted above that changes in the distribution of certain fruits and cereals, and in the dates of the harvest, have often been accepted as undoubted evidence of changes in climate. Such a conclusion is by no means inevitable, for it can easily be shown that many changes in the districts of cultivation of various crops naturally result from the fact that grapes, or corn, or olives are in time found to be more profitably grown, or more easily prepared for market in another locality. Thus the area covered by vineyards in northern Europe has been very much restricted in the last few hundred years, because grapes can be grown better and cheaper farther south. Cultivation in one district is abandoned when it is more profitable to import the product from another. It is easy, but not right, to conclude that the climate of the districts first used has changed. Wheat was formerly more generally cultivated far north in the British Isles than is the case at present, because it paid. Later, after a readjustment of the taxes on breadstuffs, it was no longer profitable to grow cereals in that region, and the area thus cultivated diminished. Changes in the facility or in the cost of importation of certain articles of food from a distance are speedily followed by changes in the districts over which these same crops are grown. Similarly, the introduction of some new plant, better suited to the local soil and climate, will result in the replacement of the older product by the newer. In France, Angot has made a careful compilation of the dates of the vintage from the fourteenth century down to the present time, and finds no support for the view so commonly held there that the climate has changed for the worse. The dates of the vintage do, however, indicate some oscillation of the climatic elements. In the period 1775–1875, the average date of the grape harvest in Aubonne was about ten days

earlier than during the preceding century, but three days later than during the second century preceding. At the present time, the average date of the grape harvest in Aubonne is exactly the same as at the close of the sixteenth century. After a careful study of the conditions of the date tree, from the fourth century, B. C., Eginitis concludes that the climate of the eastern portion of the Mediterranean basin has not changed appreciably during twenty-three centuries. In China, a comparison of the ancient and present-day conditions of cultivation, of silk production, and of bird migrations, has led Biot to a similar conclusion. In some cases, the reported cultivation of cereals, or other soil products, in certain at present unfavorable climates has been shown to be purely a myth; as in the case of a supposed extended cereal cultivation in Iceland in former times.

Secondly, a good many of the reports by explorers from little-known regions are contradictory. Thus Lake Aral, which was diminishing in area for many years, is recently reported by Berg as increasing. Lake Balkash, which was rapidly drying up, has also begun to fill again. Partly submerged trees are noted as having been seen by Berg, who in June, 1902, found the lake waters quite fresh. As the lake has no outlet, this is an interesting fact. In Africa, Lake Victoria which, it was generally agreed, was sinking in the period 1878–1892, has since shown a tendency to rise. Lake Rukwa, east of Tanganyika, has risen within the last few years. Reports that the Sea of Azov is drying up have been explained as due to a silting up of a lake. Lake Chad is very probably subject to oscillations, sometimes spreading beyond its usual limits as the result of several years of heavy rainfall. Such diverse reports show the need of caution in jumping at conclusions of climatic change. An increased use of water for irrigation may cause the level of water in a lake to fall, as has been the case to some extent in Great Salt Lake. Periodic oscillations, giving higher and then lower water, do not indicate progressive change in one direction. Many writers have thus seen a law in what was really a chance coincidence. Partsch believes that the ancient settlements on the interior lakes of northern Africa show that these lakes contained no more water formerly than they do now. Some have claimed that the supposed desiccation of the climate of northern Africa resulted from deforestation, but no certain evidence exists of the presence or destruction of such forests, and if deforestation did take place, no considerable change of climate could have resulted.

Thirdly, where a progressive desiccation seems to have taken place, the question should be asked, is less rain actually falling, or have the inhabitants less capacity, less energy, less ability than formerly? Is the change from a once cultivated area to a barren expanse the result of decreasing rainfall, or of the emigration of the former inhabitants

to other lands? The difference between a country formerly well irrigated and fertile, and a present-day sandy, inhospitable waste may be the result of a former compulsion of the people, by a strong governing power, to till the soil and to irrigate, while now, without that compulsion, no attempt is made to keep up the work. The incapacity of the present inhabitants, or of their rulers, is often responsible for effects which have been interpreted as due to climatic change. It has been shown that where irrigation is resorted to in parts of the districts about the Mediterranean which have been reported to be drying up, there the former fruitfulness returns. In many cases the reports of increasing dryness really concern only the decrease in the water supply from rivers and springs, and it is well known that a change in the cultivation of the soil, or in the extent of the forests, may bring about marked changes in the flow of springs and rivers without any essential change in the actual amount of rainfall. These conditions are particularly likely to occur in regions where there is no snow covering, and where the rain falls in a few months only. In Tripoli, the Vicomte de Mathuisieulx finds that the Latin texts and monuments seem to establish the fact that, so far as atmospheric conditions and soil are concerned, everything is just as it was in ancient times. The present condition of the country is ascribed to the idleness of the Arabs, who have allowed wells to become choked and vegetation to perish. "In a country so little favored by nature, the first requisite is a diligent and hard-working population. The Romans took several centuries to make the land productive by damming rivers and sinking wells in the *wady* beds." In an arid region, man has a hard task if he is to overcome the climatic difficulties of his situation. Irrigation; the choice of suitable crops adapted to arid conditions; steady, thoughtful work, are absolutely essential. To a large extent, an intelligent man may thus overcome many of the obstacles which nature has put in his way. On the other hand, a region of deficient rainfall, once thickly settled and prosperous, may readily become an apparently hopeless desert, even without the intervention of war and pestilence, if man allows the climate to master him.

Lastly, a region whose normal rainfall is at best barely sufficient for man's needs, may be abandoned by its inhabitants during a few years of deficient precipitation, and not again occupied even when, a few years later, normal or excessive rainfall occurs. It is a very striking fact that the districts from which comes most of the evidence of changes of climate within historical times are subtropical or subequatorial, *i. e.*, they are in just those latitudes in which a slightly greater or a slightly less migration of the rain-bringing conditions easily produces a very considerable increase or decrease in the annual rainfall.

It is apparent, on examining the evidence thus far at hand, that the fact of permanent, progressive changes in climate during historical times has not yet been definitely established.

Periodic Oscillations of Climate: Sunspot Period.—The discovery of a distinct eleven-year periodicity in the magnetic phenomena of the earth, naturally led to investigations of similar periods in meteorology. Numerous and varied studies along this line, extending back even into the seventeenth century, but beginning actively about 1870, have been and are still being prosecuted by a considerable number of persons, and the literature on the subject has assumed large proportions. The results, however, have not been satisfactory. The problem is difficult and obscure. It is natural to expect a relation of this sort, and some relation certainly exists. But the results have not come up to expectations. Fluctuations in temperature and rainfall, occurring in an eleven-year period, have been made out for certain stations, but the variations are slight, and it is not yet clear that they are sufficiently marked, uniform and persistent over large areas to make practical application of the periodicity in forecasting possible. In some cases, the relation to sunspot periodicity is open to debate; in others, the results are contradictory.

Köppen has brought forward evidence of a sunspot period in the mean annual temperature, especially in the tropics, the maximum temperatures coming in the years of sunspot minima. The whole amplitude of the variation in the mean annual temperatures, from sunspot minimum to sunspot maximum, is, however, only 1.3° in the tropics and a little less than 1° in the extra-tropics. There are, however, long periods during which there appears to be no influence, or at least, an obscure one, and the relation before 1816 seems to have been opposite to that since then. More recently Nordmann (for the years 1870–1900) has continued Köppen's investigation, using the mean annual temperatures of certain tropical stations, and finds that the mean temperatures run parallel with the sunspot curve, but that the minimum temperatures occur with the sunspot maxima (amplitude 0.7°). This seems to contradict Köppen's conclusion, and also the fact that the sun is hotter at a time of maximum sunspots. The latter difficulty has been explained on the ground that the rainfall and cloudiness, both of which are at a maximum with the sunspot curve, lower the temperature, especially in the tropics. It is obvious that the situation in this matter is rather confusing just at the present time, and that the relation of sunspots and terrestrial temperatures is not wholly clear. The sunspots themselves are probably not the immediate or sole control. "There seems little doubt," says Sir Norman Lockyer, 'that we must look to the study of the solar prominences not only as the primary factors in the magnetic and atmospheric changes in our

sun, but as the instigators of the terrestrial variations.' These investigations, however interesting and important they may be to astronomers and physical meteorologists, are really outside the field of climatology.

In 1872 Meldrum, then director of the meteorological observatory at Mauritius, first called attention to a sunspot periodicity in rainfall and in the frequency of tropical cyclones in the South Indian Ocean. The latter are most numerous in years of sunspot maxima, and decrease in frequency with the approach of sunspot minima. Poëy later found a similar relation in the case of the West Indian hurricanes. Meldrum's conclusions regarding rainfall were that, with few exceptions, there is more rain in years of sunspot maxima. This is to be taken only for *means,* and for a majority of stations, and is not to be expected at all stations or in every period. Hill found it to be true of the Indian summer monsoon rains that there seems to be an excess in the first half of the cycle, after the sunspot maximum. The winter rains of northern India, however, show the opposite relation; the minimum following, or coinciding with, the sunspot maximum. Many studies have been made of a possible relation between rainfall and the sunspot period, but the conclusions are not very definite, are sometimes contradictory, and do not yet warrant any general practical application for purposes of forecasting the wet or dry character of a coming year. Particular attention has been paid to the sunspot cycle of rainfall in India, because of the close relation between famines and the summer monsoon rainfall in that country. In 1889 Blanford admitted that the rainfall of India as a whole did not give evidence of the sunspot cycle in the records of the twenty-two years preceding. More recently the Lockyers have studied the variations of rainfall in the region surrounding the Indian Ocean in the light of solar changes in temperature. They find that India has two pulses of rainfall, one near the maximum and the other near the minimum of the sunspot period. The famines of the last fifty years have occurred in the intervals between these two pulses, and these writers believe that if as much had been known in 1836 as is now known, the probability of famines at all the subsequent dates might have been foreseen.

Relations between the sunspot period and various meteorological phenomena other than temperature, rainfall and tropical cyclones have been made the subject of numerous investigations, but on the whole the results are still too uncertain to be of any but a theoretical value. Some promising conclusions seem, however, to have been reached in regard to pressure variations, and their control over other climatic elements.

Brückner's Thirty-five-year Cycle.—Of more importance than the results thus far reached for the sunspot period are those which clearly establish a somewhat longer period of slight fluctuations or oscillations of cli-

mate, known as the Brückner cycle, after Professor Brückner, of Berne, who has made a careful investigation of the whole subject of climatic changes and finds evidence of a thirty-five-year periodicity in temperature and rainfall. Brückner began with the long-period oscillations in the level of the Caspian Sea. He then investigated the levels of the rivers flowing into the Caspian, and next the dates of the opening and closing of the rivers of the Russian empire, and finally extended his study over a considerable part of the world, including data concerning mean temperatures, rainfall, grape harvest, severe winters, and the like. The dates of opening and closing of Russian rivers go back in one case to 1559; the dates of vintage to the end of the fourteenth century, and the records of severe winters to about 1000 A.D. In a cycle whose average length is thirty-five years there comes a series of years which are somewhat cooler and also more rainy, and then a series of years which are somewhat warmer and drier. Brückner has found that the price of grain averages 13 per cent. higher in the wetter lustrum than in the drier. This thirty-five-year period is not to be thought of as being a perfectly systematic recurrence, in exactly that term of years. The interval in some cases is twenty years; in others it is fifty. The *average* interval between two cool and moist, or warm and dry periods, is about thirty-five years. Moreover, not only the intervals, but the intensities of the individual periods vary. The mean amplitude of the temperature fluctuation, based on large numbers of data, is a little less than 2°, which makes it greater than that obtained by Köppen for the sunspot period, and it is natural to expect it at a maximum in continental climates. The fluctuations in rainfall, also, are more marked in interiors than on coasts. The general mean amplitude is 12 per cent., or, excluding exceptional districts, 24 per cent. In western Siberia more than twice as much rain may fall in wet as in dry periods. Regions whose normal rainfall is small are thus most affected. In years of minimum precipitation they may become uninhabitable, and the population may be forced to move away, perhaps never returning, and allowing towns and irrigating works to fall to decay. Slight fluctuations in rainfall are most critical in regions having a normal precipitation barely sufficient for agriculture. The extent of land cultivated, and the returns of agriculture here fluctuate directly with the temporary increase or decrease of rainfall. A supplementary study of the newer rainfall observations for Russia and for the United States, as well as for certain stations in central Europe and eastern Siberia, has given Brückner satisfactory confirmation of his earlier conclusions in the fact that he finds a decrease of rainfall over these districts as a whole, beginning about the middle of the decade 1880–90. The time of the 'boom' in western Kansas and Nebraska, and in eastern Colorado,

in the decade 1880–90, followed one of Brückner's wet periods, and the collapse of the 'boom' came when the drier period advanced. Farmers who went out on to the high plains in the years of slightly greater rainfall preceding the boom, and who lost all their capital, and more too, in the vain attempt to raise their grain in the years which followed, could with difficulty be convinced that the climate of the plains had not permanently changed for the worse. The impression left upon their minds, and upon the mind of anyone who saw the country later, was one of decreasing rainfall, unsuccessful agriculture and financial ruin. Within more recent years, in this same region of Kansas, with a somewhat increased rainfall during a wetter cycle, but without any permanent change to a wetter climate, the intelligent choice of cereals better adapted to the soil and climate, and the rational use of the available water supply, have wrought a wonderful change in the aspect and economic value of the state.

The following table shows the dates and characters of Brückner's periods:

Warm	1746–1755	1791–1805	1821–1835	1851–1870	
Dry	1756–1770	1781–1805	1826–1840	1856–1870	
Cold	1731–1745	1756–1790	1806–1820	1836–1850	1871–1885
Wet	1736–1755	1771–1780	1806–1825	1841–1855	1871–1885

Interesting confirmation of Brückner's thirty-five-year period has been found by Richter in the variations of the Swiss glaciers, but as these glaciers differ in length, they do not all advance and retreat at the same time. The advance is seen during the cold and damp periods. Supan has pointed out that the Brückner periods appear to hold good in the south polar regions. And Hann's study of the monthly and annual means of rainfall at Padua (1725–1900), Klagenfurt (1813–1900) and Milan (1764–1900) brings to light an alternation of wet and dry periods in harmony with the thirty-five year cycle. It should be noted that Brückner has found certain districts in which the phases and epochs of the climatic cycle are exactly reversed. These exceptional districts are almost altogether limited to marine climates. There is thus a sort of compensation between oceans and continents. The rainier periods on the continents are accompanied by relatively low pressures, while the pressures are high and the period dry over the oceans, and *vice versa*. The cold and rainy periods are also marked by a decrease in all pressure differences. It is obvious that changes in the general distribution of atmospheric pressures over extended areas, of the great centers of high and low pressure, are closely associated with fluctuations in temperature and rainfall. An oscillation of a few hundred miles one way or another may mean the difference between drought or plentiful rainfall over extended areas. These changes in pressure distribution must in some way be associated

with changes in the general circulation of the atmosphere, and these again must depend upon some external controlling cause or causes. W. J. S. Lockyer has called attention to the fact that there seems to be a periodicity of about thirty-five years in solar activity, and that this corresponds with the Brückner period. This longer cycle, underlying the sunspot period, alters the time of occurrence of the sunspot maxima in relation to the preceding sunspot minima. He makes out three periods in solar activity, of between three and four years, about eleven, and about thirty-five years, respectively. These are related as 1:3:9.

It is clear that the existence of a thirty-five-year period will account for many of the views that have been advanced in favor of a *progressive* change of climate. A succession of a few years wetter or drier than the normal is likely to lead to the conclusion that the change is permanent. Accurate observations extending over as many years as possible, and discussed without prejudice, are necessary before any conclusions are drawn. Observations for one station during the wetter part of a cycle should not be compared with observations for another station during the drier part of the same, or of another cycle.

Climatic Cycles of Longer Period.—There are evidences of longer climatic cycles than eleven or thirty-five years. Brückner calls attention to the fact that sometimes two of his periods seem to merge into one. Richter shows much the same thing for the Alpine glaciers. James Geikie, in Scotland, has brought forward evidence of several climatic changes in postglacial times. Blytt, in Norway and Sweden, finds some botanical evidence of four great climatic waves since the last glacial period. Brögger estimates that a mean annual temperature between 3° and 4° higher than the present was found in the Christiania Fjord in postglacial time. Lorié, in Holland, finds confirmation of Blytt's views. Gradmann, on botanical evidence, believes in a warmer climate in central Europe after the last ice age, and then a cooler one. Clough concludes that a three hundred-years cycle exists in solar and terrestrial phenomena, the thirty-six year cycle being as it were superimposed upon the longer one. Kingsmill reports a periodicity of three hundred years in droughts and famines in northern China. And so on. As yet, nothing sufficiently definite to warrant discussion here has been brought forward.

Geological Changes in Climate.—Changes of climate in the geological past are known with absolute certainty to have taken place: periods of glacial invasions, as well as periods of more genial conditions. The evidence, and the causes of these changes have been discussed and re-discussed, by writers almost without number, and from all points of view. Changes in the intensity of insolation; in the sun itself; in the conditions of the earth's atmosphere; in the astrono-

mical relations of earth and sun; in the distribution of land and water, in the position of the earth's axis; in the altitude of the land; in the presence of volcanic dust—changes now in cosmic, now in terrestrial conditions—have been suggested, combated, put forward again. None of these hypotheses has prevailed in preference to others. No actual proof of the correctness of this or that theory has been brought forward. No general agreement has been reached. Under these conditions, and in view of the fact that practical climatology is concerned with climatic changes, not of the geological past but of the historical present, this portion of our subject may be dismissed with this brief mention.

Conclusion.—There is a wide-spread popular belief in permanent, progressive changes of climate during a generation or two. This belief is not supported by the facts of meteorological record. Abundant evidence has been adduced in favor of secular changes of climate in historical times. Much of this is unreliable, contradictory, and has been interpreted without sufficient regard to possible controls other than climatic change. Without denying the possibility, or even the probability, of the establishment of the fact of secular changes, there is as yet no sufficient warrant for believing in considerable *permanent changes over large areas.* Dufour, after a thorough study of all available evidence, has concluded that a change of climate has not been proved. There are periodic oscillations of slight amount. An eleven-year period has been made out, with more or less certainty, for some of the meteorological elements, but it has been of no practical importance as yet. A thirty-five year period is less uncertain, but is nevertheless of considerable irregularity, and can not as yet be practically applied in forecasting. Longer periods are suggested, but not made out. As to causes, variations in solar activity are naturally receiving attention, and the results thus far are promising. But climate is a great complex, and complete and satisfactory explanations of all the facts will be difficult, perhaps impossible, to reach. At present, indeed, the facts which call for explanation are still in most cases but poorly determined, and the processes at work are insufficiently understood. Climate is not absolutely a constant. The pendulum swings to the right, and to the left. And its swing is as far to the right as to the left. Each generation lives through a part of one, or two, or even three, oscillations. A snap-shot view of these oscillations makes them seem permanent. As Supan has well said, it was formerly believed that climate changes locally, but progressively and permanently. It is now believed that oscillations of climate are limited in time, but occur over wide areas. Finally, it is clear that man, whether by reforestation or deforestation, by flooding a desert or by draining a swamp, can produce no important or extended modifications of natural climate, which is governed by factors beyond human control.

THE AFRICAN PYGMIES

By S. P. VERNER

THE presence of a group of the African pygmies at the World's Fair at St. Louis attracted considerable attention to these little people. It has also revealed a number of erroneous popular conceptions with reference to them.

The word pygmy, of course, comes from the Greek, being derived from the word denoting a unit of measure, the ell. It was used by various Grecian writers, among them Homer, Herodotus, Heliodotus and Aristotle, to describe a race of small men, about whom tradition had given accounts, and who were usually located toward the sources of the Nile. Historically, then, the word pygmy applies to these Nilotic small peoples, but anthropology has widened the use of the term to include similar peoples scattered all over the globe, and found in many parts of Africa.

Paul du Chaillu was the first eminent modern explorer to find these people. He discovered them in the upper Ogowe basin, west central Africa, in July, 1863. After him others found them in various places. These were Schweinfurth, 1869, on the upper Welle, or Ubangi; Wissmann, 1886, on the upper Kasai; Stanley, 1888, on the upper Aruwimi; while Dr. Donaldson Smith located some south of Abyssinia. Others report them in German Kameruns, in French West Africa, on the borders of Uganda and in the center of the Congo Basin.

The names by which these people are called vary in each locality, but the most widely used term is Batwa. The name Bantu is the word meaning people in a large area of Central Africa. The singular of this is Muntu, meaning a man. These two terms apply to the large or normal people, not to the pygmies. But curiously enough, the name Batwa is the plural for people with the Batwa pygmies, and the singular of this is Mutwa. These last two terms seem to the writer to be diminutives of the words Bantu and Muntu, so that they mean little people and little man, respectively. Sir Harry Johnston, who visited the pygmies in the region where Stanley first found them, spells the name for them Mbute, while Schweinfurth, whose pygmies are not far from those of Stanley and Johnston, calls them Wambutti. It seems to the writer that these are either variations in name or in spelling of the same word. The present governor-general of the Congo, Major Costermans, found some Batwa near Lake Kivu. Wissmann's pygmies

On the left is Bomashulba, a Batwa Pygmy, Age 25 years, Height 4 ft. 7 in., Weight 90 lbs. On right is an ordinary African, Bakuba Tribe; Name, Latuma; Age, 15 years; Height, 5 ft. 2 in. Weight, 135 lbs.

in the upper Kasai are also called Batwa, which is the way Stanley spelled the word for his little neighbors, although these regions are six hundred miles apart.

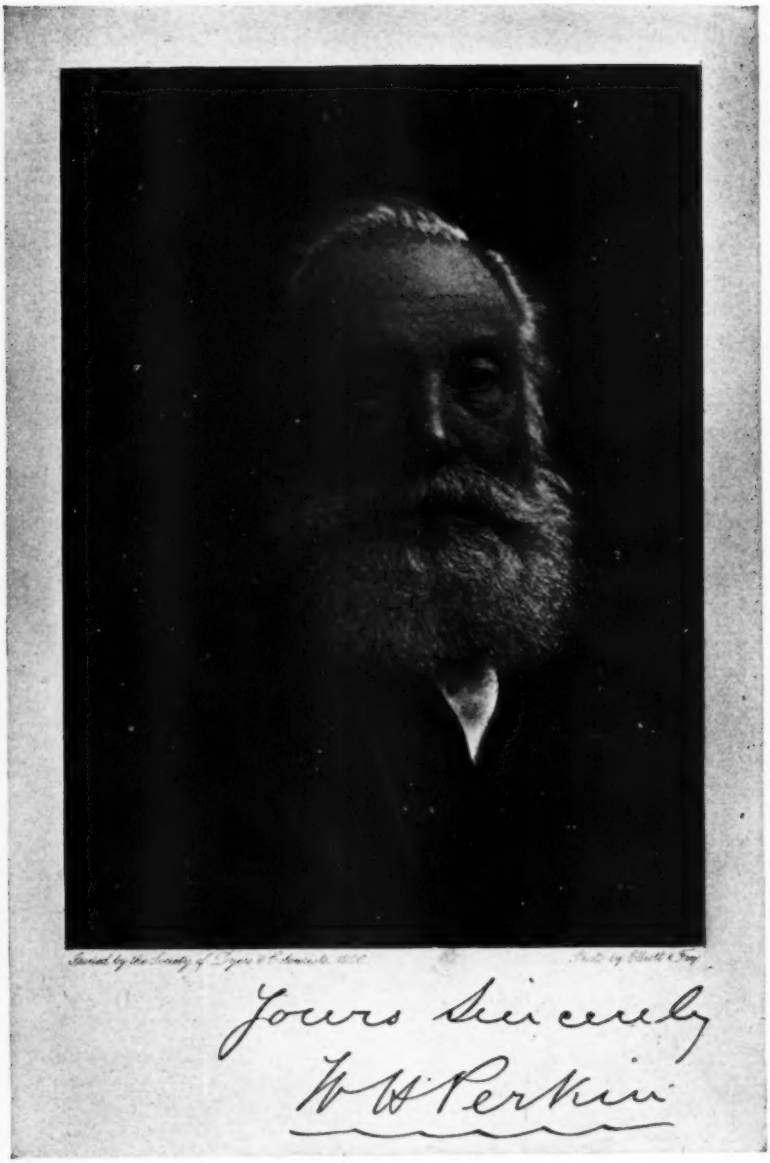
A study of all the writings of explorers reporting discoveries of these people has revealed an average stature of about four feet eight inches for all measured, though the measurements were made upon so few that this average can not be relied upon as a final result.

The plant which furnishes the leaf covering for the huts of the pygmies is the same in the regions, widely apart though they are, explored alike by Stanley and Wissmann. The shape of the house—a rough hemisphere—is also the same. In practically every case the primitive culture of the pygmies is the same, wherever found. The lack of any agriculture in their life is a common characteristic, as are the use of the poisoned arrow and the lack of any centralized tribal organization.

Popular misconceptions about the pygmies are principally as to their height. The general idea having gone abroad that they are the smallest known race of man, there has been produced the impression that they are all veritable Tom Thumbs. Of course, anthropologists know better that this, but the layman can not get clear the difference between a dwarf and a pygmy. Then, too, some travelers have rather unscientifically measured the smallest they could find, and left this as the record of the height of the tribe.

Dr. Mason, of the Smithsonian Institution, and Professor Starr, of Chicago University, concur in making five feet as the limit for the average of a pygmy race. Of course, there will be a few taller than this, and many shorter. It will also be necessary to discriminate against any result of the admixture of alien blood from larger tribes, although there is comparatively little of this going on.

The group at St. Louis came from the region in which the Batwa were found by a number of explorers, though their particular settlement was visited only by the writer and the Reverend W. H. Sheppard, F.R.G.S. These other explorers who found the Batwa in the upper Kasai are the English missionary, Grenfell; the German explorers (under the Congo government), Pogge and Wolff; and Major von Wissmann, who ranks next to Stanley as the explorer of the Congo Basin, and who was subsequently the governor-general of German East Africa. The pygmies at St. Louis were from the forests near Wissmann Falls, the cataracts at the head of the navigation of the Kasai tributary of the Congo. This place is about a thousand miles in the interior of the continent. There are a number of Batwa settlements in the same general district.



Issued by the Society of Dyers & Colourists, 1906. Photo by Elliott & Fry.

Yours sincerely
W. H. Perkin

THE PROGRESS OF SCIENCE

THE JUBILEE OF THE COAL TAR COLOR INDUSTRY AND SIR WILLIAM HENRY PERKIN

It is not often that a scientific man can take part in the celebration of the fiftieth anniversary of a great discovery that he has made. Perkin, when he was a boy of eighteen, noted a muddy precipitate which occurred in connection with an attempt to produce quinine artificially. The new idea that this substance could be used as a dye-stuff, the courage which led to a patent and further investigations, and the patient persistence with which all practical difficulties were overcome are an unusually clear exhibit of what is meant by genius. This violet color was called 'mauve' and was the forerunner of the aniline dyes and of much besides.

Fifty years ago the dyeing and printing industries were on an extremely empirical basis, the natural dye-stuffs being applied by secret and rule-of-thumb methods. In order to substitute a laboratory product and scientific methods, it was necessary for Perkin to establish a manufactory, which he did with the aid of his father and brother. The production of aniline was put on a commercial basis, and the discovery and application of other dye-stuffs was a comparatively simple matter. Magenta was discovered in France three years later, and subsequently all the colors of the rainbow were produced from aniline, Perkin himself being largely responsible for alizarine, and indirectly for synthetic indigo. As is well known, the artificial production of these dye-stuffs has led to great changes in agriculture and manufactures, one of the most important being the alliance of science and industry in Germany, which has given that country almost a monopoly in the work that originated with Perkin in Great Britain.

The coal tar products now give us flavors and perfumes as well as colors. Saccharine, more than five hundred times as sweet as sugar, was discovered in the laboratory of the Johns Hopkins University. Salicylic and benzoic acids, though natural products, are chiefly produced artificially. Although quinine, which was the subject of Perkin's original research, has not been made by synthesis, the coal-tar products have given us an extraordinary series of drugs—antipyrin, acetanilid, phenocoll, etc. They have given us smokeless powder, photographic films and indirectly nitrates from the atmosphere and the cyanide process of gold extraction.

The scientific advances have been no less remarkable than the industrial developments, and it should be especially noted that to these Perkin has contributed his full share. After acquiring a competence, he sold his manufactory in 1873 and has since devoted himself to scientific research. Professor Nernst has stated that Perkin is the founder of physical chemistry. In connection with his influence on chemistry, it should be remembered that two of his sons have become eminent for work in organic chemistry, both being fellows of the Royal Society.

The scientific and industrial developments following on Perkin's great discovery were adequately celebrated by an international gathering at the Royal Institution on July 26. It was presided over by Professor R. Meldola and various speeches and presentations were made, including the Hoffman and Lavoisier gold medals and degrees from

several foreign universities. There was also a dinner, at which Mr. Haldane, secretary for war, proposed the toast of the evening and addresses were made by Professor Emil Fischer, Sir Henry Roscoe and others. Sir William and Lady Perkin held receptions at their home near Harrow and at London. American chemists decided to hold a special celebration of the jubilee, and Sir William and Lady Perkin accepted an invitation to be present at a banquet to be given in New York City, on October 6. Professor Chandler, of Columbia University, presided, and addresses were made by Dr. Hugo Schweitzer, President Ira Remsen, Dr. H. W. Wiley, Professor Walther Nernst and others. Sir William Perkin was presented with the first impression of a Perkin medal, which will hereafter be awarded annually for work in applied chemistry; with a silver tea service, and with honorary membership in the American Chemical Society. There will also be founded in honor of Sir William Perkin a circulating library for American chemists.

THE ROYAL SOCIETY.

SIR WILLIAM HUGGINS, eminent for his contributions to spectroscopic astronomy and president of the Royal Society from 1900 to 1905, has collected into a volume, published by Messrs. Methuen and Company, selections from four of the addresses given at anniversary meetings of the society. The addresses are prefaced by a sketch of the early history of the society, and are followed by a discussion of the place of science in education. The volume contains excellent copies of paintings of fellows of the Royal Society which hang in its halls, and other illustrations, several of which are here reproduced.

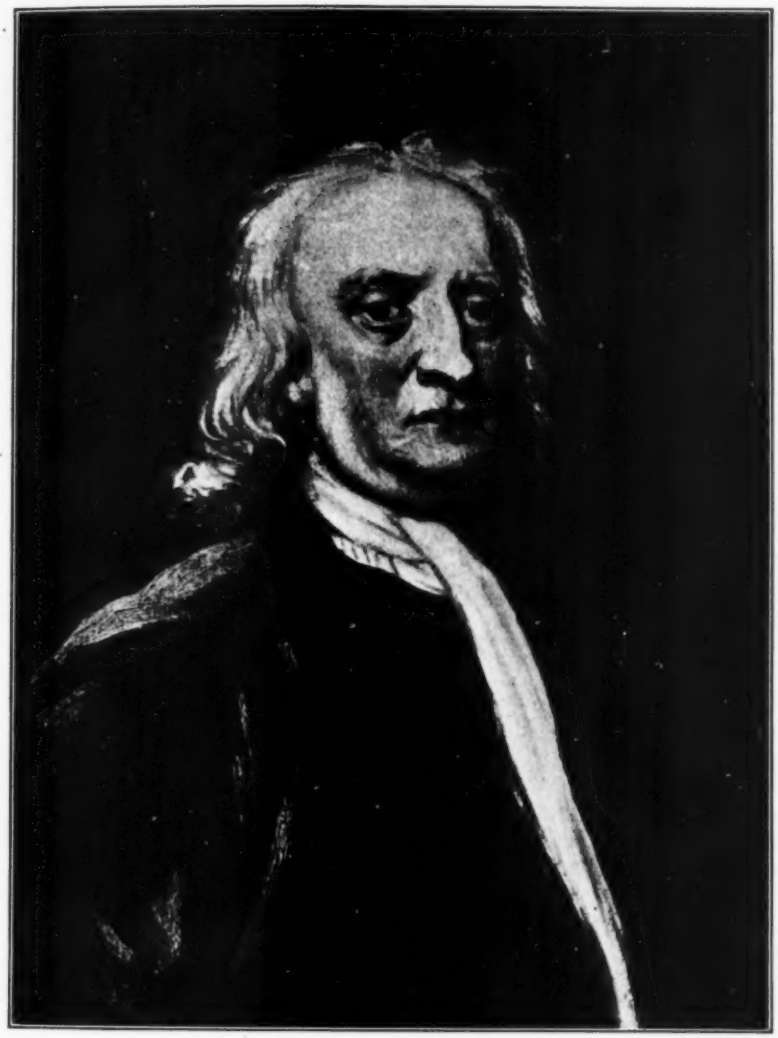
REFLECTING TELESCOPE made by Newton with his own hands in 1671, standing on the bound MSS. of the *Principia*.

DEATH MASK OF NEWTON.

The address of 1902 is concerned with the importance of science to the industries of Great Britain, which, it is argued, can only be maintained by making science an essential part of all education. Germany and the United States are quoted as examples that should be followed in their encouragement of research work as a condition of academic degrees. The solution offered appears to be rather optimistic, for Sir William wishes to retain the

culture to be derived from classical and literary studies, and to find time for the thorough study of science by improving methods of teaching and by greater devotion to work on the part of the students.

This address had a practical outcome in so far as the council of the Royal Society was lead to draw up a memorandum urging the universities to give greater encouragement to science with a view to its recognition in schools and elsewhere as an essential part of general education, but no very considerable results followed, both Oxford and Cambridge having voted shortly after the presentation of this letter to reject plans for the acceptance of a larger amount of science in the entrance examinations.



SIR ISAAC NEWTON.
PRESIDENT OF THE ROYAL SOCIETY, 1703-27.
From the painting by T. Vanderbank.

MEETING ROOM OF THE ROYAL SOCIETY IN BURLINGTON HOUSE.

The second address, given in 1903, reviewed the relation of the Royal Society to the special scientific societies. The increase of knowledge and the necessary differentiation of the sciences led to the foundation of the Linnean Society for natural history in 1788 and to the establishment of the Geological Society in 1807, and there are now in London and elsewhere in Great Britain numerous societies devoted to the special sciences. Sir William does not discuss the relations of the Royal Society to the British Association for the Advancement of Science, nor to the British Academy for philosophy, history and philology, which was established during his presidency, after long discussion as to whether the subjects that it covers should be included in the scope of the Royal Society. It appears that efforts were made some ten years ago to form a more or less close affiliation of the principle special societies with the Royal Society, but the plan did not prove feasible. Sir William favors publishing the papers read before the Royal Society both in its transactions and in the publications of the special society, and such a plan is in operation in the case of the Royal Astronomical Society. It is not, however, clear just what is gained by this plan for science, though it might for the time lead scientific men to present their papers before the Royal Society. This discussion has led to the publication of the transactions in two parts, one for the natural sciences and the other for the exact sciences, but it is difficult to see the advantages of publishing, even in two series, papers scarcely one tenth of which would be of interest to any one student. It does not seem that meetings at which papers in all the sciences are read have a useful function at the present time, and this appears to be clearly indicated by the programs and attendance at the ordinary meetings of the Royal Society and of our own National Academy of Sciences.

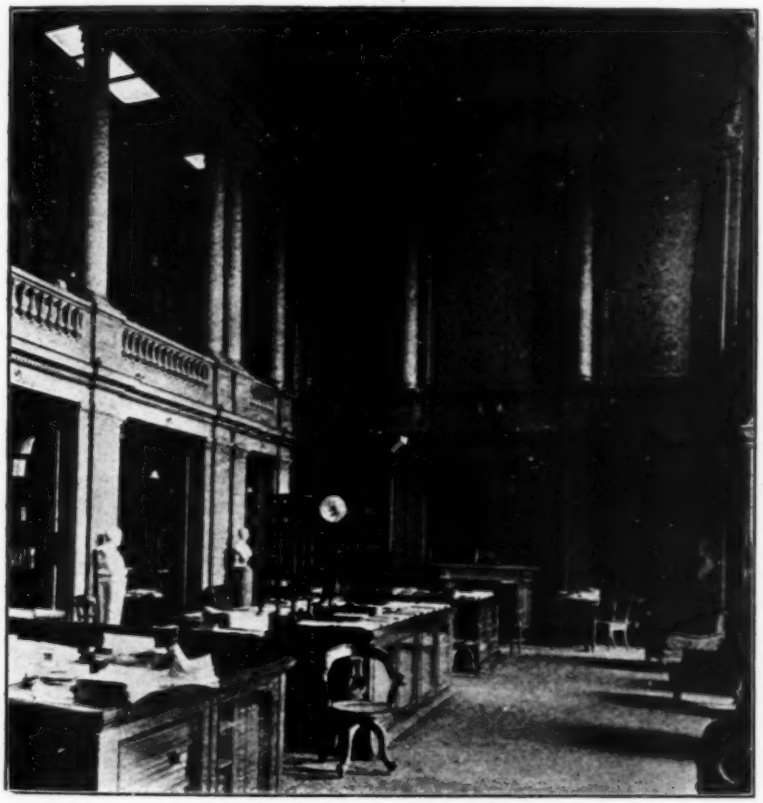
The third address discusses the relation of the Royal Society to the state and its responsible public duties. The society is not supported by the state, although it administers a government

fund for research and publication amounting to £5,000 annually and occupies rooms provided by the government. Sir William takes pride in the fact that the society is a private body of learned men for the promotion of natural knowledge at their own cost. Certainly the British traditions have been well maintained by the Royal Society, Sir William himself being a notable example. He has devoted his life to astronomical research, has built his observatory at his own cost and has worked without a salary of any kind. It is, however, somewhat doubtful whether such traditions can be maintained in the future. In any case, they belong to an aristocracy rather than to a democracy. It will probably be found that even in Great Britain the government will need to employ its scientific men, and will not depend on the voluntary advice and assistance of an independent society.

The fourth address reviews the influence that science, represented by the Royal Society, has had upon the life and thought of the world. It is truly remarkable what a large part of the great scientific advances from Newton to Darwin, and since Darwin, have had their origin in the work of the fellows of the Royal Society.

THE YORK MEETING OF THE BRITISH ASSOCIATION

THE British Association for the Advancement of Science has celebrated its foundation in York seventy-five years ago by meeting this year in that city. The attendance at the meeting was 1,959, which is fully as large as the



THE PRINCIPAL LIBRARY OF THE ROYAL SOCIETY.

average, though some 600 fewer than at the meeting in the same city twenty-five years ago, when Sir John Lubbock presided and the fiftieth anniversary of the foundation of the association was celebrated. The president this year was Dr. E. Ray Lankester, director of the British Museum of Natural History. His address consisted of two parts—one a survey of the progress of science during the preceding twenty-five years; the second a discussion of the relations of the government to science. It is an almost impossible task for one man to describe the extraordinary and diverse scientific advances of the past generation. Although the address as printed is very long, numerous important topics are of necessity omitted, and others are perhaps unduly emphasized. It is not certain that Metchnikoff's phagocytic theory of immunity occupies the important place in modern science that is given to it in this address, nor even that radium twenty-five years hence will loom as large as it does now. In his review of the influence of science on the life of the community and its relation to the government, Professor Lankester takes the somewhat pessimistic attitude which appears to be common in Great Britain. He says that political administrators are altogether unaware of the vital importance of science in public affairs and that whole departments of the government in which scientific knowledge is the one thing needful are carried on by ministers and clerks who are ignorant of science and dislike it. Dr. Lankester attributes this ignorance and dislike to "the defective education, both at school and university, of our governing class, as well as to a racial dislike among all classes to the establishment and support by public funds of posts which the average man may not expect to succeed by popular clamor or class privilege in gaining for himself—posts which must be held by men of special training and mental gifts."

Dr. Lankester then enumerates on the other side of the account the establishment of the National Physical Laboratory, the subsidizing of the Marine Biological Association and the endowment of the Lister Institute by Lord Iveagh. He continues: "Many other noble gifts to scientific research have been made in this country during the period on which we are looking back. Let us be thankful for them, and admire the wise munificence of the donors. But none the less we must refuse to rely entirely on such liberality for the development of the army of science, which has to do battle for mankind against the obvious disabilities and sufferings which afflict us and can be removed by knowledge. The organization and finance of this army should be the care of the state."

The British Association will meet next year at Leicester under the presidency of Sir David Gill, astronomer royal in South Africa. The meeting of 1908 will be in Dublin, and in 1909 the association will for the third time visit Canada, meeting in Winnipeg.

SCIENTIFIC ITEMS

DR. LUDWIG BOLTZMANN, eminent for his work in theoretical physics, of which subject he was professor in the University of Vienna, has committed suicide.

PROFESSOR S. F. EARLE has resigned the directorship of the Cuban Central Agricultural Station, which was organized in 1902.—Dr. H. C. Wood, for thirty years professor of therapeutics in the University of Pennsylvania and until 1902 clinical professor of diseases of the nervous system, has retired from the active duties of his chair, and has been made professor emeritus.—Dr. A. R. Crook, for the past twelve years professor of mineralogy and economic geology at Northwestern University, has been appointed curator of the Illinois State Museum of Natural History at Springfield.